A.I. KITAIGORODSKY

I AM A PHYSICIST

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FOREWORD

Professor Alexander Kitaigorodsky, Doctor of Science in physics and mathematics, is well known not only among his professional colleagues but to a wide circle of readers as well, for he is an ardent popularizer of science.

In *I Am a Physicist*, Prof. Kitaigorodsky takes up a difficult but worthwhile task: that of introducing the young reader to the world of science and showing the world surrounding him through the eyes of a physicist. He has succeeded remarkably in this endeavour. A scientist in love with his profession, he conveys the complexity of the problems faced by contemporary physics, the intensity of scientific quest and the excitement involved in it with flare and imagination.

If there is one thing Alexander Kitaigorodsky does not lack it is versatility. The range of problems he raises is truly staggering. What is the measure of patience? Is telepathy possible? Is money spent on scientific missions and conferences justified? What is the relative importance of the natural and applied sciences? What does

the word "explanation" mean? What, if any, are the differences between man and machine? How has physical thinking changed since Aristotle's time? How should a young man go about choosing a profession?

And these are but some of the problems he touches upon. However, for all his diversity of approach, Prof. Kitaigorodsky has a definite singularity of purpose, which is to reveal to the reader the role of physics in natural science and the role of natural science in science and life.

Chapter 1

WAYS AND GOALS

...declares that the purpose of science is to explain nature and subordinate it to man. Natural science has the explaining task. The ways it takes and goals it pursues are the subject of this chapter.

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In the course of millions of years of evolution Nature has nurtured in man the urge to create things, all kinds of things: houses, hatchets, hoes, highways, and what have you. When this instinct, inherited from the remote shaggy forebear fitting a suitable stick into a hole in a piece of stone and hacking a cosy three-room cave in a rock overlooking a picturesque river valley, is dominant, its possessor becomes an inventor, engineer, agronomist. If, in addition, such a person is not inclined to restrict himself to repeating the achievements of his predecessors, if he feels like building new kinds of houses, of creating tools no one ever used before, of reaping harvests undreamed of before, then he joins the vanguard of the builders of civilization.

A man who contributes appreciably to the world's advance towards spiritual wealth and comfortable life must possess many qualities. He must know the successes that have been achieved and the setbacks suffered before him. He

must possess an excellent memory capable of absorbing all the facts directly or indirectly bearing on the matter in hand. He must be stubborn and tenacious in pursuing his goal. Edison tried hundreds of materials before he found a suitable filament for his electric bulb.

People of this frame are driven by fervent, unquenchable desire to see the fruits of their work. They know no peace until millions and millions begin to benefit from their efforts. And they must be able not only to work, but also to prove that they are right.

It is no easy task to prove the need to reorganize industry, or replace a material in common usage, or change over from old transport means to new ones.

The conservatism and caution of society in such instances is only natural, and distrust of innovations is part of human nature. That is why it is not enough to be knowledgeable and capable: one must also learn to stand up for one's work and be a clever diplomat capable of outmanoeuvring the opponents of innovation.

Ultimately the better always (or almost always, to be on the safe side) wins out. This is a law of life. But at what cost of nerve and stamina on the part of those whose sole desire is unselfishly to make life easier and better for others! Unselfishly? But of course! The proof? Try to offer money to a man to give up his task before he has finished it. Nothing doing: recreants are not easily found. I've known many such men, and I never cease to marvel at the ardour with which they serve their cause. They encounter many disappointments on the way to their

goal, but all the greater the joy when success finally comes. Public recognition is ample recompense for all one's worries and anxieties, for toil oblivious of the passage of time, for utter devotion to one's work. To many the word "science" is associated with just this kind of work.

That was what I, too, thought for a long time. In the home of my father, who had devoted his life to the creation of new materials, knowledge was evaluated from the point of view of practical utility. One had to study, one had to acquire knowledge to be able to make new machines, create tougher steel, construct faster airplanes.

Boys are often asked what they want to be. I too was asked, and I invariably replied without a moment's hesitation, "I'm going to work in science".

"What science?"

"I don't know yet, but I'll search for ways of building houses of glass or else I'll improve electrical machines."

Later on I learned that many people had other ideas about the meaning of the word "science".

I was fourteen or so when I began to visit regularly the Rumyantsev Library (the old building of the Lenin Library of today). At first I read books connected with the school curriculum.

At the geography class the teacher speaks about India. Very well. That means I must read something about Hindu fakirs or the habits of rajahs.

At the literature class the Decembrists are mentioned. That's also interesting: must get myself Yakushkin's memoirs.

Soon, however, the choice of books began to depend less on school than on opinions voiced in the library's smoking room. I must confess that I was already smoking at that tender age. The smoking room was the library's club, and I suspect that many visitors never even got as far as the reading rooms. It was always noisy there, boys shouted and argued. They argued heatedly, impassionedly, jumping from one subject to another. There wasn't a topic they didn't discuss: Darwin and religion, hypnosis and autosuggestion, Blok's attitude to Belinsky and Meyerhold's stage productions.

Today I don't regret having begun smoking early. My visits to the smoking room are among my best recollections, and whereas it would be an exaggeration to say that I received my education there, at the very least the endless debates led to my reading of books which would otherwise have remained unknown to me.

One of the frequenters of the smoking room was a young man by the name of Valery, if I remember correctly. He had long hair plastered down to his head, a gaunt face and dark, deep-set eyes. He smoked only the cheapest cigarettes and never went to the snack-bar. He apparently had no money, but he was extremely neat and tidy. His boots, though patched, were brightly shined, and he tried to modernize his out-of-fashion jacket by fastening the skirts with a safety pin (the fashion then was for jackets to drape the hips tightly). I remembered him not for his striking appearance, however, but for his impassioned speeches, especially on the subject of man's vocation. It seemed only natural

to Valery to classify human aspirations by their importance, and it was from him that I first heard ideas on science that differed essentially from my home-bred notions.

"Utilitarianism is an invention of bourgeois society," he declared, puffing a cigarette. "It was Francis Bacon who preached that the quest for knowledge was meaningless if it served no practical purpose.

"I can just see a pot-bellied shopkeeper admonishing his son," Valery went on, emphasizing his speech by jabbing a cigarette clenched between a thumb and forefinger. "Who cares for poetry, painting or useless knowledge about the number of stars in the universe? Only several hundred cranks like you. But all people need and want clothes, they dream of a warm, cosy home. Help them achieve this, and you will gain respect, honour and wealth."

Valery dragged at his cigarette.

"One would think that any intelligent man should be nauseated by such a frankly philistine conception. But look you... People are afraid to object, more, they would place this so-called philosophy at the root of every creation of the human spirit. It's disgusting to hear Bacon declare that if poetry has any use at all it is merely as a recreation after work, or that abstract knowledge is useful only insofar as with time it may be turned into cheap sausage. How can one say such things? How can one reduce all human aspirations to the desire to fill one's belly? Only science and art divorced from any practical gains are capable of deifying man, of lifting him above animal level."

"You're speaking nonsense!" others shouted at him. "A hungry man cares nothing for pure art, he hasn't the slightest concern for the size of the atom. We must feed people first and then worry about their soul."

Still, rarely did anyone rise in defence of Bacon. Others agreed with Valery — in principle. They just considered his concerns and worries untimely.

"To be sure," they said, "society doesn't prevent you from engaging in art or pure science. But don't expect the ardent gratitude of the present world, in which millions of people are still short of food and shoes. When the Golden Age comes..."

Once, when I happened to be alone with Valery, I asked him timidly, "What are the goals of science, if not the creation of useful things?"

"The purpose of science," he retorted without hesitation, "is the discovery of new facts and connections between phenomena. Examples?"

I nodded.

"Very well. Here are examples from different fields. A physicist finds that the electrical conductivity of copper decreases with the increase of temperature. This is a new discovery, a new fact, which enriches science. A student of Australian fauna discovers a new species of ant. This is another example of an indubitable gain of science. After sifting through a vast number of original documents a historian establishes that the Great French Revolution was received hostilely among the German burghers — this is another example for you."

"And this is the purpose of science?" I asked with wonder.

"No. The establishment of new, hitherto unknown facts is merely the basis of science. The more capable, the more far-seeing, I should say, researchers have the job of building the upper floors of the edifice of science. They use the facts to discover generalities and establish the connections between phenomena, that is, they discover the laws. Say, after accumulating a great body of data on the electrical conductivity of various materials a scientist observes that they can be classified into two groups: those whose conductivity increases with the temperature, and those whose conductivity decreases. Next, it is found that these two classes differ in the electron structure of their atoms. These special laws, in turn, gradually accumulate and become a basis for wider conceptions concerning the connection between the electrical properties and structure of matter."

"Well, but what about biology or history?"

"The same thing. Without the painstaking work of an army of researchers it's impossible to discover the general laws of evolution of the animal world, the laws governing the development of human society."

"I see. But tell me, Valery, if a man is looking for ways of improving the quality of steel, isn't he engaged in science?"

"I would find some other word for this kind of work," he said thoughtfully. "Of course, often many new and interesting things are discovered here too, but this is not a purpose in itself."

"Do you mean that you've less respect for this kind of work?" Everything within me protested, I rose in defence of my father. But it wasn't necessary.

"Goodness, no," he said calmly. "It's most fine and praiseworthy work. To prove its usefulness would be like breaking into an open door. The thing is that very few people realize that the selfless quest for knowledge is as equally deserving an undertaking."

It was then that I realized that people could attach different meanings to the word "science".

Hence, it is important for us to agree on what we mean by the word.

I have perused newspapers and magazines, listened to the radio and sought the views of people utterly divorced from science in whatever sense. I find that the word "science" has expanded tremendously. When it is said of a person that he is a scientist or is engaged in science it usually means no more than that he possesses a certain body of knowledge which he uses in his work. It is a minority that attaches a narrower meaning to the word: to them engaging in science means seeking the new and unknown. Yet even such a definition is too broad. To avoid misunderstandings, let us divide scientific activity into three domains: the applied sciences, the humanities, the natural sciences.

Any research in the sphere of applied sciences pursues a practical end. The development of a new manufacturing technique or improvement of an old one, the creation of new materials, the introduction of faster and more convenient transport means, increasing soil fertility, the

creation of new drugs: these are all tasks being daily handled by engineering physics, engineering mechanics, chemical technology, agronomy, medicine, etc.

The trends of development of the applied sciences are fairly obvious. They are guided by the practical needs of mankind, the needs of the nation. Power reserves are running out: men look for new energy sources, and the nuclear industry appears. Wool is in short supply and is too expensive: scientists hunt for cheap and efficient substitutes, and the manmade fibres industry appears.

And so it goes.

Man lives in a world of material things. How can he best subordinate them and make them work for him? The answers to this are supplied by the applied sciences.

But why do things behave like this and not in some other way? How are events in the world of things interlinked? How is the world about us arranged? These questions are answered by the natural sciences. Their ultimate purpose is an understanding of nature.

The division into natural and applied sciences is not a division by branches of knowledge. Physicists who study the laws of scattering of elementary particles; chemists who study the laws governing the breaking of chemical bonds; biologists using an electron microscope to study the structure of a bacteriophage's flagellum — these are all workers in the same detachment. A physicist designing a nuclear reactor; a chemist improving the properties of polythene; a selectionist producing a new animal breed —

these, according to our classification, all belong to another army.

But perhaps the classification is arbitrary? Is it not simpler to treat all physicists as physicists and all chemists as chemists? No, no, and no! Far from being simpler, it could lead to misconceptions.

The classification according to branches of knowledge remains largely among researchers engaged in applied science. Our subject, however, is natural science. And we shall never be able to understand the basic trends of its development if we cling to the outdated habit of building fences to separate physicists from chemists from biologists.

A characteristic feature of the last few decades is the destruction of boundaries between different chapters of natural science. Chemical reactions, biological processes and phenomena of the inanimate world are nowadays studied by the same methods and proceed from common theoretical premises.

It is one of the purposes of this book to show how all natural scientists have rallied under the same banner.

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Having explained what is meant by natural science, I should like to tell the reader what a single investigation in the field of natural science represents.

I intend to do this on the example of my speciality. But first I should like to say a few words about how I chose it.

I knew only one thing for sure: I wanted to work in science. In what science?

Gradually the negative responses began to appear.

The idea of going in for technology was probably one of the first to be turned down. Not because I thought its tasks unattractive. Far from it. Simply I felt a predilection for sober analytical speculation. I had no taste for the logical leaps in researches that are inevitable in the applied sciences, with their feverish pursuit of an ultimate goal. Nor did I experience any desire to see at once the tangible results of my labours. A new idea seemed more precious to me than a new gadget.

It was not long before I rejected the thought of devoting myself to the humanities. The vagueness and instability of judgements pronounced on the basis of meagre facts irritated me.

Biology in the days of my youth seemed no more than a chaotic assemblage of facts. There appeared to be not the slightest possibility for strict analysis; the quest for common laws governing the existence of all living creatures seemed hopeless. (Today I regret my early impression: biology is turning into an exact science before our very eyes.)

Pure mathematics did not tempt me either.

The system of action that appealed to me most was: analysis of facts, the quest for general laws, their verification by experiment. Only physics could give me all this. So physics was to be my profession!

My choice of profession was not fortuitous, it was in keeping with my general bent. The

choice of narrow speciality, however, was pure chance. It so happened that two years before finishing the university course I began to work in a laboratory, where I was engaged in the branch of physics known as "X-ray analysis of crystals".

It was a time when pure research was belittled and, furthermore, research workers were paid much less than engineers. My friends, most of whom had gone in for engineering, tended to look down upon me. Any one of them could clearly formulate the objective of his work. One was busy improving electric locomotives, one was perfecting gyrocompasses, one was designing aircraft.

They spoke enthusiastically of their work, people quickly grasped their ideas and listened to them readily. Naturally, I also wanted to speak of my achievements, and one day I told my friends of my first completed experimental investigation.

"You see, I've managed to establish the distance between the atoms in a molecule of aminoacetic acid."

"What for?" someone asked at once.

"What do you mean? Those distances were unknown till now."

"So what? Who needs the information?"

I was unable to answer them, my friends scoffed at me and I lapsed into a piqued silence.

The question "What for?" so simple in the applied sciences, turns out to be far from simple in natural science, and an answer to it requires a degree of elucidation.

Having determined the distances between the atoms of a molecule of aminoacetic acid I solved

a problem of my narrow scientific speciality, which is, as I have said, X-ray analysis of crystals. This is a very small sector on the front-line of science. And yet several tens of thousands of workers all over the world are engaged in studying crystalline structures with the help of X-rays. Their purpose is to perfect experimental and mathematical methods so that investigations could be carried out faster and more precisely, and to study the structure of as many substances as possible for all cases when it cannot be predicted in advance. X-ray analysis of crystals is concerned with the solution of these tasks. In the same way, any other scientific speciality is an activity that develops according to the goals it pursues.

There is not a member of the scientific community of physicists and chemists studying the structure of crystals who would question the need of perfecting mathematical and experimental methods of X-ray analysis or the need to determine unknown crystalline structures. Nor is this questioned by researchers working in other spheres of science.

What is it for? The correct answer is the one given by my library companion: to elucidate facts still hidden from science. Every newly established fact (however insignificant it might be) and every new point of view (however negligible the phenomenon to which it refers) is bound, sooner or later, to be of use. In a year or in several decades they are bound to be of use to some other worker. The achievements of science may be handed on and on through dozens of scientific papers by dozens of authors, and they will ulti-

mately reach the stage when they begin to pay off and become a component of a major discovery or accomplishment.

I could mention that without knowledge of the structure of graphite it would have been impossible to design a nuclear reactor. And without improving the mathematical apparatus of X-ray analysis it would have been impossible to establish the structure of the gene and, hence, approach an understanding of the nature of heredity.

Thus, the work of people dealing in X-ray analysis of crystalline structures is an essential element in the advance of science as a whole.

The example is purely arbitrary. One could trace a similar participation of any other section of natural science in major scientific and technological breakthroughs: infrared spectroscopy or calorimetry, the theories of luminescence or adsorption; the mathematical theory of the Fourier transformations or the theory of relativity. And these sections of science, like science as a whole, solve problems of their own. But every scientific investigation introducing something new, albeit at first apparently insignificant, may, through a great number of intermediaries, eventually become part of a great and important discovery.

Take any scientific paper: at the end you will find a bibliographic list. The author acknowledges the work of other scientists from which he gleaned some ideas or where he discovered new facts. Even a small paper has on average some 20 bibliographic references. As a rule none of the authors of these 20 works had the slight-

est idea who might find their investigations useful. Twenty is the number of works the researcher has acknowledged. But how many unacknowledged scattered ideas and facts taken from other works have also gone into the groundwork of his investigation!

Every good research work (and a good work is one which has resolved some of moot points) dissolves in the works that follow it. Grains of the ideas of one author are present in hundreds and thousands of works of researchers who have read his paper. Just as letters form words and words form sentences, so individual researches form new scientific ideas, topple or erect new hypotheses or pave the way for scientific discoveries.

How unalike are works of science and art in this respect! A work of creative art is complete in itself and can be evaluated according to all the common criteria applicable to art as a whole. If art is likened to an imposing building, an artist's work can be compared with a similar building, though very small, but with the same windows and doors...

No scientific investigation is ever complete in itself. It acquires meaning only thanks to the work of predecessors and followers. If science is compared with a great building, then an individual research is a brick in its walls.

As the ages pass art accumulates its values, discarding the mediocre, preserving the great. Not as a museum relic. The work of a great artist stirs the imagination of listeners or viewers hundreds and thousands of years after it was created.

The road of science is straight. The ideas of every researcher, the facts he has acquired are a step on that road. Without that metre of asphalt there is no road, but once past it the road proceeds onward. Look back: the marked place grows smaller and smaller in the distance until it finally disappears from sight altogether.

The lifetime of a work of a "scientist-author" is discouragingly short, perhaps no more than thirty to fifty years. This is the time it takes to dissolve thoroughly in the works of other researchers: the best it contains is absorbed, the superfluous is cast aside, the work itself becomes a well-squeezed lemon.

This is the fate not only of rank-and-file researchers. This is the fate of books and works by the greatest physicists: Newton, and Maxwell, and the quite recent Einstein. Their works interest only the historians of science. Our knowledge of the work of geniuses reaches us through recapitulations by our contemporaries. Time polishes the greatest discoveries, gives them new form, sometimes changing them tremendously. Present-day mechanics — the brainchild of Newton — only remotely resembles the mechanics written by Sir Isaac.

Perhaps this digression will help you to understand why it is so hard to assess the value of an isolated scientific investigation.

I must confess that when a natural scientist recounts his achievements to the lay public he is compelled — I should not say to exaggerate, but to generalize and in effect describe not the significance of his own contribution to science, an all but hopeless task in lay language, but the

significance of the whole field in which he works.

Obviously, there are exceptions when, against the background of the daily plodding humdrum of research work, we can discern fairly clearly the birth of a new idea, the discovery of a new phenomenon, the creation of a new research methodology. The significance of such a new breakthrough may be apparent at once. This was the case, for example, with the work of the American scientists Lee and Yang, who discovered a new property of elementary particles, or the work of the German physicist Mössbauer, who discovered a new effect in the interaction of gamma-rays with matter. These scientists were almost immediately awarded the highest distinction for scholarly research, the Nobel Prize.

But this is rather the exception. Much more numerous are the examples of belated recognition.

In 1934, a post-graduate student by the name of Cherenkov discovered a new phenomenon in the scattering of electrons in liquids and solids. At the time no one could foresee that many years later it would lead to the creation of excellent nuclear radiation counters. The work won recognition, and with it the Nobel Prize, a quarter of a century after it was carried out.

Not so long ago our outstanding theoretical physicist Landau was awarded the Nobel Prize for works carried out before the last war.

In 1945, Zavoisky in Kazan discovered resonance absorption of radio waves by electrons. At the time it was impossible to foresee any extensive development of his work, nor the emergence of a new sphere of physics: the study of

matter by magnetic resonance. This work, too, gained recognition long after it was carried out.

That is why it is not so easy to evaluate the work of a researcher in the sphere of natural science.

It is much simpler to assess the work of an engineer shop superintendent. The quantity and quality of output is measurable in tangible figures, and a girl from the quality inspection department can readily say what shop works better.

The value of a scientific worker in the domain of applied science is immediately apparent. He produces a new material — and its advantages over the existing ones can be judged. He elaborates a new manufacturing technique — and figures will show how much better it is than the old one. In such cases a bookkeeper's assessment is adequate proof.

And in the natural science domain?

I am frequently asked to review scientific papers, dissertations, and reports on research works. How do I form an opinion of them?

The main thing is to determine the degree of novelty. A work that says nothing new is not worth the paper on which it is written. The novelty need not be striking. Say, a researcher employed conventional apparatus and well-known methods, but in connection with new objects which had not been studied before. Should the work have been done? Doubtlessly. But it does not deserve much praise, even if the experimenter worked a lot to obtain the results.

More deserving is an author of a new method of measurement or a new method of calculation

(if, obviously, the methods are faster or more accurate than the old ones). A review of such a work may even be concluded with several flattering sentences praising the author for his skill and ingenuity.

The compliments become superlative when a new phenomenon has been discovered, or a new correlation or rule enabling the outcome of an experiment to be reliably predicted is evolved theoretically.

So far I have been dealing with sufficiently objective criteria. When one undertakes to assess the significance of a research work one usually has to rely on intuition to decide how important the discovered facts and rules may prove to be, how they may promote the advance of science as a whole — things which usually become apparent within years, if not decades.

The conclusion to which I am leading the reader is simple enough: the battle front of natural science is wide and continuous, and thousands of researchers contribute to its advance. Each one facilitates this advance by establishing new facts in his or her work.

Very well. But what is the purpose of natural science as a whole? To cognize the world and discover new things. What for?

Fridtjof Nansen's answer to such a question was:

"The history of mankind is an endless striving from darkness to light. That is why it is useless to discuss the goals of knowledge: man wants to know; when this urge passes he will stop being man."

Man's thirst for knowledge needs no explanation: it is a thirst for the joys of life.

Scientific creativity is one of the most selfless of human activities, it is among the most wonderful human emotions. Many vivid confessions of these joys could be cited. Here is an often quoted passage from the works of Ptolemy:

"I know that I am mortal and my existence is Grief. But when I study the stellar multitudes, my feet no longer rest on the Earth, I stand next to Zeus, partake of the food of the gods and feel myself a god."

In truth, understanding of nature, knowledge of her secrets, ability to predict a phenomenon in all its details fill a man with a sense of tremendous pride, with tremendous joy, help his self-assertion. Nothing is more capable of overthrowing god than learning and knowledge. Man who knows nature feels himself its creator, feels himself omnipotent and does not require spiritual support.

Thus, one does not have to explain why man studies nature. But there is another important question that must be answered. Has a natural scientist the moral right to pursue his science in the modern world, in which there are still so many millions of hungry, suffering people?

Is it not his duty to devote his knowledge and abilities to serving the practical needs of the present day?

No, a worker of "pure" science need not suffer from pangs of conscience. The advance of natural science leads to one technological revolution after another, thereby tremendously accelerating man's approach to universal affluence. For that reason the work of scientists is the concern of the whole of the society in which they live.

Chapter 2

A DISCOURSE ON THE USE OF SCIENCE

...in which the author, supporting his arguments with facts from his biography, seeks to convince the reader that the natural sciences, with their goal of knowledge of the world, are very useful.

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In 1936-38, when I was just launching my scientific career, one of the leading institutes of physics was the Leningrad Physico-Technical Institute. It was headed by Abram Ioffe, a fine scientist and organizer, a man whose role in the establishment of Soviet physics can hardly be overestimated. Perhaps half of the country's living leading physicists are in one way or another pupils of Ioffe or come from his institute. At the time in question the institute was subordinated not to the Academy of Sciences but to the People's Commissariat of the Engineering Industry. The Commissariat was in Moscow and, accordingly, it was in Moscow that plans were agreed, appropriations and personnel were obtained and all kinds of administrative problems were settled. It was necessary to be in constant contact with the Commissariat, and Ioffe felt that he had to have someone to speak for his institute's interests, to act so to say as his envoy in Moscow.

By a happy stroke of luck his choice fell on me,

and I was thus able to witness the development of researches at his institute. True, my "ambassadorial" duties were short-lived. I have forgotten exactly, but I think the institute was subordinated to some other organization, making my services unnecessary. However, that brief period was sufficient for me to see the foresight of Ioffe, who resolutely advocated the development of research in fields which at the time did not appear at all promising.

I well remember my visits to the Deputy People's Commissar or department chief with the institute's plans. Armed with Ioffe's explanations (I had travelled several times to Leningrad to see the institute's work on the spot), I had no difficulty in convincing the practically-minded administrators of the need to develop semiconductor physics. Although this branch of physics was still in an embryonic state at the time, its potentialities could be demonstrated graphically with the first semiconductor photoelectric cells. I brought the small, coin-like devices to the Deputy Commissar's office and connected them to a measuring instrument. I brought the cell close to an electric bulb, and the needle of the instrument deflected sharply; then I placed a piece of ebony between the bulb and the cell, but the current was only slightly weaker.

"You see," I summed up the experiment, which in our time is demonstrated at school, "the photoelectric cell is sensitive to infrared rays."

This was so convincing that sums for developing the work of laboratories concerned with

the miraculous photoelectric cells were issued without a murmur.

The trouble began when the chief's pencil running down the list reached the nuclear physics laboratory: Ioffe was insistently demanding money for a cyclotron.

"What's it for?"

"The splitting of the atomic nucleus is one of the most exciting pages of contemporary physics."

"It costs too much to fill these exciting pages," the administrator said doubtfully. "You can tell that nothing practical can come of laboratories that deal in billionths of a gram of matter. You can't base technology on such tripe."

There was nothing to say to this. At the time no one had the slightest idea of the ways any practical purposes could be served by work in nuclear physics. Sober prejudice could be countered only with faith in the power of science. Arguments in favour of the development of nuclear physics were basically of the kind advanced by our outstanding mechanic and ship-builder Alexei Krylov:

"A blast furnace produces 500,000 tons of pig iron a year; a cyclotron of about the same size and cost yields a 100,000th of a milligram of split atoms. However, within my memory the only practical applications of electricity had been the electric telegraph, electric bell and galvanoplastics. And now! The forces and power of science are unlimited, and just as unlimited are its practical applications for the benefit of man."

Fine, prophetic words, clouded only by the fact that research in nuclear physics has culmi-

nated not only in atomic power plants, but in the atom bomb as well...

The history of natural science abounds in examples of scientific discoveries that have had a revolutionary impact on the development of civilization. Suffice it to recall Faraday's discovery of the law of electromagnetic induction, which became the foundation of all electrical engineering and, hence, of the whole of contemporary civilization. In this case, too, the import of the discovery was utterly unappreciated at the time it was made. I have read an anecdote somewhere according to which, when Faraday was asked of possible applications of his law, he replied that it could probably be used to make quaint toys.

There are countless examples of a so to say lower order: Roentgen's discovery of penetrating rays, the discovery of the photoelectric effect, the discovery of rubber synthesis...

It is important to realize that all these and other scientific discoveries were not fortuitous revelations; they were the outcome of the logical and natural evolution of science.

It is utterly childish to imagine that Roentgen had "looked" for his invisible rays, or Faraday for natural laws which could be used for building electricity generators, or Hahn and Strassman for atomic energy. At the same time, though, it is wrong to think that Roentgen was "lucky" because a mineral that glows under the action of what later became known as Roentgen or X-ray happened to be lying near the gas-discharge tube covered with a black paper he was experimenting with. One could perhaps claim

that Faraday was just lucky when he happened to glance at the right moment at the needle of the galvanometer connected with a wire coil just as he was inserting a bar magnet into the coil. One might say that Hahn and Strassman were just lucky when they discovered, in 1939, that slow neutrons broke uranium nuclei in two; the true portent of this discovery — the possibility of an atomic explosion — became apparent several months later.

In real fact — and the history of science can always prove this beyond a shadow of doubt — these discoveries were prepared by the work of many thousands of researchers. They became possible because they were ripe, inevitable, they were in the air. The keen eye of the most talented scholar discerned them before others.

Here we could end our discourse on the use of science. The need to develop the front of science, pushed forward by man's curiosity, by his desire to understand nature, to remove all that is vague or incomprehensible from the world, make all eventualities predictable in the eyes of even the most hardened utilitarian — this need finds justification in the fortunate inevitability of major scientific discoveries. Without the advance of the whole front of science, without the efforts of the whole army of unknown toilers of science, these discoveries would have been impossible.

This alone is sufficient to understand why, in our country, the development of theoretical works in physics, mathematics, chemistry and biology is regarded as a matter of state concern and why appropriations for the development

of natural science are on a par with other state expenditures.

Most readers, I hope, will be satisfied with the above reasoning. Nevertheless, I should like to pursue the subject with an eye on the minority, among which there may be more or less educated, practically-minded skeptics.

"You claim," such a skeptic may say, "that revolutions in technology are linked with scientific discoveries. True enough. You have offered some very satisfactory examples. But allow me to cite some examples of an opposite nature. Quite a few branches of technology achieved a high degree of perfection long before natural science ever appeared. Our distant forebears, who had not the slightest idea of the laws of physics or chemistry, nevertheless could build imposing castles, make exquisite crystal glass and smelt metals. The age of steam began without the participation of science. Watt and Polzunov knew nothing of the laws of thermodynamics, which treat of the transformation of heat into work. Or take the manufacture of steel or glass. What wealth of useful materials have been created by experimental search and are not the upshot of scientific analyses or the study of the laws of nature. Thus, "the skeptic concludes," the practitioners have managed to cope with their problems without the help of theoretical science."

It is certainly true that many spheres of technology were born and perfected without the help of science. However, when natural science began to make substantial gains and its ideas impregnated practically every applied science without exception, the traditional techniques from which,

it had seemed, everything useful had been squeezed, received a new lease of life and began to develop at a faster rate. Though the history of steel manufacture dates back many centuries, it was only in the late nineteen-fifties that a new process was suggested that yields steel three or more times tougher than before. One need not go into the importance of this innovation. Engineers strive to reduce the weight of structural components by a few per cent; the new steel makes possible a reduction of the weight of machine parts by ten, twenty and more per cent.

Thirty years ago theoreticians put forward ideas concerning the reasons why metals are not as hard as might be expected. The thing is that the crystals of metals possess observable specific imperfections known as dislocations, which tend to propagate in the crystal on the application of a slight force. When the dislocations are numerous a crystal deforms when subjected to small forces.

In the early fifties detailed diagrams of the propagation of dislocations were worked out and methods of recording and observing the propagation of dislocations on individual crystals were proposed.

Metals experts followed these works closely, hoping to find in them an answer to the question of how to make steel harder. The theory of dislocations gave an unequivocal answer: the propagation of dislocations had to be halted.

The metallurgists and metal physicists began to wonder how this could be done. Here is one example of their reasoning on the basis of dislocation concepts. It is known that small amounts

of carbon turn soft iron into hard steel. The part played by the carbon became clear: its little atoms keep dislocations from propagating. Hence, it is not a question of the chemical nature of the additive, and carbon can be successfully replaced by other elements.

The idea, and the experiments, moved from the physical laboratories to the metallurgical institutes, and from there to the factories. The complete cycle took about a decade.

One could cite many such examples, in which the "introduction" time of new ideas ranges from a year to several decades. This is not our task, however. The important thing is to show that in a country where the natural sciences are highly developed the applied sciences — engineering, medicine, agronomy, etc.— are in advantageous conditions. Such a country will sooner come to the practical utilization of a scientific discovery than otherwise. Moreover, the general culture of scientific thinking has a profound influence on all practical affairs.

The moral of this chapter is: Although the natural sciences advance along roads of their own and solve no practical problems, their effect on the applied sciences can hardly be overestimated.

Chapter 3

WE ARE NOT ON AN UNINHABITED ISLAND

...which shows how the dialectical unity of freedom and necessity determines the trends of research in the natural sciences.

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Almost every week a man working in science must stop to ponder over the ever recurring question: What next?

A laboratory assistant wonders whether perhaps he should use a more sensitive film. A researcher decides to make a pause in his experiments and check his figures against theoretical data. The head of a group investigating a common field decides that the time has come to introduce a new observation methodology, he devises new measurement schemes and sends his blueprints to the workshop. The laboratory chief considers that the time has come to shift the emphasis from optical to radiospectroscopic methods of investigation, that new experimental curves suggest the need to revise old theories, that the substances studied must be supplemented with new items. And the director of the institute is concerned (at least when he is sitting in his administrative office) with the allocation of money and assignments to the laboratories.

It follows from the above scheme that a unified, coordinated line of research is pursued

on the laboratory level. Larger units are of an administrative character, smaller ones are not independent. (Obviously, it is not a question of name, and as often as not it is a tiny team of researchers or even an individual who functions as a laboratory.)

A good laboratory (we shall call a laboratory any independent research group) has its lines of work, its circle of interests and its style of research. One need not name the authors of a paper produced by a good laboratory: the specialist will immediately recognize the source.

A research unit may be in the making. It may lack a distinctiveness of its own. Such a state is completely legitimate for five to seven years. But if a laboratory continues to lack distinction a decade after its organization, this is an indication that it is mediocre and doesn't deserve to be rated as a unit on the scientific front. Such a laboratory may serve auxiliary purposes, if some other unit assumes patronage, using it for its own researches.

The lines along which a scientific collective pursues its studies and the style of its work are determined by its leader or by a small group of senior workers. The laboratory's name is not very revealing: it defines only the general field in which it works. Laboratories with the same name can and must differ in style of work and the lines they pursue as much as different theatre companies.

In what ways may styles differ? First of all in attitude towards laboratory experiments. Some laboratories devote much effort to building complex apparatus and devising precision me-

thods of measurement. In other laboratories the researchers prefer to buy equipment so as to devote all their efforts to the processing and interpretation of measurements. Some laboratories embrace wide fields of research, others concentrate on the details of a specific problem.

Style and line of pursuit evolve gradually as a sum of many factors: the leader's temperament and mentality, the impact of general scientific development and advances in adjacent fields, the influence of industrial and national interests.

A scientific leader's role is decisive in drawing up the research plans. In natural science there is no such thing as centralized planning. State control is restricted to the distribution of funds amongst different fields of science in accordance with the current notion of their relative importance.

A laboratory chief cannot draw up his plans in the same way as an industrial executive: more often than not he just can't plan the results he will obtain.

In institutes of the Academy of Sciences, for example, each year a laboratory submits its plans for the following year to the management. And each time the workers are at a loss when they have to fill in the standard plan forms with their listings of theme, breakdown of the work by stages and anticipated results.

It is not hard to state what we intend to do, what measurements we intend to carry out, what apparatus we would like to have installed, and what experiments we hope to stage. But will it all be done?

Naturally, research work abounds in routine. One can readily estimate the time needed to take an X-ray picture or obtain a spectrum, one can say how much time it will take to carry out a calculation. It is harder, though not altogether impossible, to indicate how many weeks it may take to build an apparatus according to known blueprints. But a research work that consists solely of such operations is no good, it's not a work of research.

Scientific research has meaning only if it is undertaken to unravel something unknown or vague. An experimental work is the better the less apparent its eventual outcome. That which seems simple and easy may turn out in the course of the investigation to be startlingly complex; on the contrary, a tangled problem may prove to have a simple solution.

Surprises? Yes. But then, they are probably the main thing in science. Every researcher dreams of stumbling on a surprise. A surprise is something new, something no one had ever encountered before. Surprising, interesting, important are synonyms in science.

Last spring I was giving final instructions to Yusif, a post-graduate student of mine, before going on holiday.

"Your work is coming to an end, Yusif. All that is left is to demonstrate that the speed of molecular processes in a solid decreases in repetitive experiments. [It seemed obvious to me that the crystals with which Yusif was working must gradually deteriorate.] Measure the rate at which the speed of the process decreases, and with that I shall consider your work completed."

I left. When I returned a month later I went at once to Yusif.

"Well, show me your graphs."

"Here they are."

"Wait a minute, you must be mistaken."

"No, I'm not."

"Where are the curves of decreasing speed? I see bell-shaped curves."

"That's just what they are."

Can you imagine! It appeared that the speed first increased and only then decreased. That was an unexpected result. The inference was that the crystal at first "got used" to the molecular process and only then began to deteriorate. Yusif had discovered a new phenomenon, which added immeasurably to the value of his work. And naturally, the plan of research had to be drastically revised.

This is one example of how hard it is to plan research in natural science. I should even say that the more one is compelled to depart from his original plans the more interesting his work.

Whenever I go through the research plan forms which college teachers are obliged to fill I can't help smiling. The column "Subject" is followed by "Number of printed pages". I can readily understand the psychology of the man who compiled these forms. A teacher's plan stipulates the number of hours he will devote to lectures, seminars, examinations, consultations. It is not hard to check the fulfilment of this plan against class and course registers. But what about research work? Plan the number of hours? How does one go about verifying them?

It's even worse with theoreticians. "I work at home," he declares. So what should his plan stipulate? Perhaps the number of pages in a scientific treatise? After all, you can count them...

The foolishness of such an approach is obvious. Accounts of masterful research works appear occasionally in papers printed in the "Proceedings of the Academy of Sciences". The "Proceedings" accept articles of no more than six standard type-written pages in length. As often as not, these six pages embody years of work and mental effort that defy evaluation by any units of measurement. On the other hand, how many mediocre, bulky four-hundred page dissertations have I had occasion to leaf through (there is no use reading them).

Scientists know only too well how impossible it is to plan the results of research work. That is why everyone is used to see the column "Expected results" virtually repeat the column "Content of work". Still, the management, understandably, desires to know what a laboratory intends to do this year, and what it hopes to accomplish.

As mentioned before, basically the choice of subjects to be tackled in the coming year rests with the head of the laboratory; the decisive measure of their value is his understanding of what is most important and interesting in the scientific domain to which he has devoted his life. At the same time, a laboratory leader must bear in mind the overall trend of work in the institute to which his laboratory belongs. Otherwise he will have to endure polite but persistent reproaches at annual progress report

sessions, and the material pressure exerted by the institute authorities will force him to reckon with the overall interests of the organization to which the laboratory belongs. If a laboratory's understanding of the degree of importance of various themes appears erroneous to the scientific council, the laboratory will be criticized, generally to its benefit. That a laboratory takes the right line depends first of all on the intelligence, talent and insight of its head.

Research work is also carried out under college departments and chairs, which enjoy greater freedom in their choice of subject matter. The reason is simple enough: a college's main task is training good experts, and pedagogical work is strictly controlled. As for scientific work — if it is conducted, all good and well, if not— all the head of the department or chair has to do is to learn to compile suitable answers to the questions concerning the number of pages written by his subordinates. That, alas, is all.

Research work in the natural sciences is carried out mainly by the so-called general departments: physics, chemistry, biology. The head of such a department is free to choose any topic whatsoever as a field of investigation.

However, we are not living on an uninhabited island and the demands of life must inevitably influence the choice of field of investigation of beginners, and the lines along which to pursue an investigation in the case of established scientists.

The problem of freedom and necessity is resolved, as elsewhere, in dialectical unity. Psychological and material factors usually induce

a researcher standing at a scientific crossroads to tackle problems confronting applied science.

Examples abound. The tremendous practical importance of semiconductors is common knowledge. Hence the rapid development of the relevant chapters of solid-state physics.

The physics of elementary particles has expanded so greatly because the original researches in the field resulted in the discovery of atomic energy.

Investigation of the structure of high polymers would never have progressed so rapidly if not for industrial interest in synthetic materials.

Or an example from my own laboratory. Although it specializes in the structure of organic compounds, we were always cool on high-molecular organic substances: it is hard to produce them in a highly ordered state and therefore harder to study their structural characteristics.

However, in the nineteen-forties the words "high polymers" began to be repeated more and more frequently. Chemists called to gain information on the structure of high polymer compounds. We answered some questions and were stumped by others, which made us think about the laws governing the structure of these compounds.

Gradually the natural course of events drew us into the domain of new problems posed by practical applications. It is natural for man to want to feel himself useful to as many of his fellow-men as possible, to feel himself a direct participant in the implementation of the tasks facing the state. Alongside such psychological

pressures there also develops a purely pecuniary interest: the possibility of obtaining expensive equipment and additional floor space "for the study of polymers", and thus to expand the scope of work.

Examples of the accelerating effects of practical considerations on research in natural science are very numerous. Still, in some cases the researcher resists this pressure. For example, when a change in the orientation of one's work is to the detriment of one's scientific qualifications.

It is considered self-evident that a researcher must choose his scientific domain once. Here is a small digression to illustrate my idea. It was like this. The war interrupted my scientific studies. The institute where I had worked before the war ceased to exist, and when the time came for me to return to my profession I had to seek a new place of work.

I am restless by nature and I always regretted being tied to one place: participation in an expedition or a mine inspection tour doesn't fall within the terms of reference of a physicist concerned with the structure of matter. So, I decided, since I had to begin all over again anyhow, I might as well go in for marine physics. The study of sea currents and surf phenomena presented an excellent opportunity for simultaneously quenching my thirst for scientific creativity and satisfying my urge to be on the move. Accordingly, I called at a laboratory of marine physics. They accepted my papers and asked me to come for an interview with the head of the laboratory on the following day.

He was most kindly and helpful.

"But, my friend, this is excellent, you are such an experienced structuralist [there is such a jargon word], a candidate of science. I'm delighted and I'll certainly take you. You will study the structure of ice..."

This was so unexpected that I didn't even bother to explain my motives for seeking the job. I suddenly realized that it simply couldn't occur to a person that I could throw ten years of knowledge and experience overboard like so much ballast. I had to reconcile myself with a speciality not involving travel.

It is rare indeed that a scientist forsakes his profession. Not only because it seems a pity to discard all the scientific knowledge you have acquired. The scientific domain, the line of research you have chosen very quickly becomes your labour of love, and parting with it involves a painful break-up.

Not always is devotion to one's profession rewarded. Some ride the crest of rising waves, others plod on unobtrusively and are denied opportunities for expanding their work.

There are cases, of course, when the road leads into a blind alley. This is especially sad. However, for the most part even unobtrusive research makes its necessary contribution to the impetus of scientific advance. Sometimes the course of events may lead to a reappraisal of values and the hitherto unnoticed suddenly find themselves in the front ranks. This, to take one example, was the case of the nuclear physicists. This is now happening before our very eyes with researchers working in the field of molecular biology.

It is only natural that practical considerations have a decisive effect on researchers standing at a scientific crossroads (I am repeating myself, but the truth gains from repetition), just as it is natural for researchers to pursue their road doggedly for the sole reason that departing from it would mean betraying the cause to which they have devoted their lives.

I have spoken of the impossibility for a researcher to change his allegiance within the realm of natural science. It is equally rare for a natural scientist to cross over completely to the camp of applied science.

No one challenges an artist's or a poet's right to follow his vocation. A natural scientist's vocation is just as powerful, and it also runs in his blood.

There is a category of people possessed with the urge to attack the unknown and deriving tremendous satisfaction from the possibility of foreseeing future events.

I would very much like to make the reader feel how exciting and interesting this is. You have conceived a theory on the basis of which you have, for example, calculated how the heat capacity of spar depends on temperature. You have carried out a great deal of work and plotted a theoretical curve, a beautiful smooth line climbing up from low temperature, first slowly, then rapidly, then slower again as it approaches the limit. The time is now ripe to verify the theory. It is not so easy to build the required apparatus. Week after week passes, and your impatience steadily mounts. Is your theory correct or not? Have you learned to forecast the

event? At last the apparatus is ready and you commence your measurements. The first point, the second, the third... They fall right on the curve! What joy, what triumph! You leave the laboratory late at night, a foolish, happy smile lighting up your face, you are in a state of euphoria resembling that of a young lover going home after a date with his beloved.

For many people the study of nature aimed at filling in the blank spots on the map of science develops into a passion, into the purpose and meaning of their life. Need one say that such people inevitably find themselves in the front ranks of the army of science.

Every researcher naturally wishes to give greater scope to his work and, naturally, he never has enough money, enough room, enough assistants. Give him a free reign and he will purchase all the best equipment there is in the world and, of course, reinforce his two devoted technicians with at least two small workshops, a mechanical shop with a staff of twenty or so, and an electrical shop with a mere handful of ten to begin with. The researcher in love with his profession notes with displeasure, if not anger, that other investigations incomparably less significant than his own work have received greater appropriations. Obviously, this is because people do not realize the true import of his work. But never mind, give him a little more time, he will produce new results and then one and all will at last see the importance of his line of research.

A man devoted to his science, capable of strict, logical reasoning when it concerns the analysis of scientific facts, completely loses his objecti-

vity when he has to promote the cause to which he is devoted heart and soul, to which he has devoted his life, nay, which is his life! I must say that I admire this loss of sense of reality, this egotism of the highest order, this covetous passion.

The desire to expand his work as greatly as possible, to obtain more money and better apparatus, forces the researcher to balance his work so as to satisfy practical demands without detriment to his main scientific work. He devotes a portion of his laboratory's time and energy to solving problems of industry or applied institutes. For this the laboratory gets funds and equipment enabling it to carry out its basic scientific tasks more efficiently.

The supplementary financing of science through a system of contracts with industry is extremely useful. As a result industrial enterprises carrying out important practical tasks resort to the help of and encourage the work of the very theoretical laboratories whose scientific affairs are most successful, the ones which work best. In short, a system of automatic feedback evolves in which the good laboratories obtain additional sums, and this is only just.

Chapter 4

WE HAVE A COLLOQUIUM TODAY

...which describes how a researcher keeps abreast of the achievements of science all over the world. The author seeks to demonstrate that trips to distant countries on scientific business do more than simply satisfy one's curiosity.

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We have a colloquium today. It is now 2:27 p.m. Time to go. Being late is not permitted, and Rimma, the secretary of the colloquium, is already rattling the piggy-bank into which the late participants obediently drop ten-kopeck pieces: one for every minute they are late. In a year's time there'll probably be enough money for a good dinner party. Discipline is lacking among the staff and the piggy-bank gains weight steadily.

Academician Kapitsa manages to have his colloquiums start exactly on the dot. His "Wednesdays" are the most representative scientific gatherings of physicists in Moscow. Kapitsa maintains a strict schedule. His colloquiums not only start on the dot, they also end with an accuracy of half a minute, lasting exactly two hours. If a report extends beyond the time Kapitsa politely interrupts the speaker in the middle of a sentence, declaring that it is all very interesting and we shall be happy to hear

the end next time. It is worse when the subject has been exhausted and there are still five or ten minutes to go. But Kapitsa is a skilled helmsman and, manoeuvring with questions and recollections, he steers the ship into port exactly on the hour. Not a minute late, not a minute early.

I haven't learned the trick yet, and our meetings last only approximately two hours. More than two is useless as people tire and their attention wanders.

A laboratory or institute colloquium is the connecting tissue that joins the separate cells into a scientific organism. A researcher works alone or with a few subordinates; in any case he does his brainwork alone. This is inevitable. But contact is also essential. Carried away by one's own line of action and reasoning, one may well lose sight of important things, follow the wrong track and discover something already known. It is impossible to do successful work without an idea of the place and importance of your endeavour in science. Of course, one can (and should) read a lot. But the devoted scientist finds it harder to get away from his work to read than to discuss a point. Besides, reading can't substitute for a living exchange of views. Scientific literature usually doesn't mention failures. A paper is written when something has been achieved. That you are embarking on an erroneous road you can find out only in a conversation.

At laboratory colloquiums we hear reports on the work of both members of our staff and guest speakers invited from other laboratories and institutes. The speaker expects criticism

or approval, advice and help. Having made two or three reports at representative colloquiums without hearing sarcastic remarks to the effect that, firstly, all he has said is trivial, that, secondly, it has long been published and that, thirdly, it contains gross errors, the researcher knows that he can go on safely with his work. The listeners take note of the new information they hear and reflect on possible applications in their own fields.

This part of a colloquium's work is the most important and most interesting, but it is not enough. One must, nevertheless, keep track of world scientific literature.

I can't help sighing as I write these words. One may well speak of keeping track of world literature. Our scientific forebears of the 19th century could do it easily enough. Once a month they waited impatiently for the appearance of the one or two scientific journals in their field. Two or three days of reading was sufficient to keep abreast not only of new developments in physics or chemistry but of the general achievements of natural science as a whole.

Those good old times are gone forever. The rapid expansion of scientific research defies the imagination. One lover of statistics estimated the number of scholars that lived on earth from the days of Romulus till our time. He found that ninety per cent of them are our contemporaries. A century ago the number of research workers was in the thousands; today there are millions. It seems likely that in the third millennium every tenth inhabitant of the globe will be engaged in science.

The results of the labours of this scholarly army are reported in scientific journals. Do you know how many are published today? Fifty thousand! If their publication was spread out evenly you would be receiving a new magazine every ten minutes. In 1960, they carried 1,200,000 articles. So you can understand the reason for my heavy sigh: a million papers in all languages, from Spanish to Japanese.

How can one expect to keep abreast of the achievements of science? One must, obviously, reject the idea of knowing everything about all sciences. Specialization has, alas, become inevitable. In fact, it has become impossible to follow all developments even in physics alone.

In science, the magazine industry is not centralized. Far from it. Numerous journals in many countries deal with identical subjects. Even more handle overlapping topics. Where, for instance, could a paper entitled, "Investigations of the Infrared Spectrum of Hemoglobin Crystals in Connection with Certain Problems of Their Structure", be expected to appear? In the Soviet Union alone such magazines as *Solid State Physics*, *Journal of Experimental and Theoretical Physics*, *Optics and Spectroscopy*, *Crystallography*, *Structural Chemistry*, *Biophysics*, *Biochemistry* and a host of others would all be legitimately entitled to publish it.

"How do you manage?" the wondering reader may ask.

One way out is the existence of various digests, abstracts and synopses, the importance of which is steadily rising.

In my line we have a journal of physical abstracts. Each month I get a thick volume containing summaries of 3,000 papers. I spend two or three evenings leafing carefully through it, concentrating on articles bearing directly or indirectly on the laboratory's work, as well as on abstracts devoted to general problems of natural science. Out of these I select a dozen or two of the most interesting for our colloquium.

The purpose is dual. Acquaintance with the papers the abstracts of which seem interesting takes time. But know them you must. So let our new workers report on them at the colloquium. The other purpose is also important. Junior workers must learn to speak at scientific gatherings. Summarizing someone else's work is excellent practice. The inexperienced speaker is not so flustered since he is not responsible for the results and conclusions and all he has to do is look after the form of his report, which is simple enough.

Journals of abstracts are fine things. If you follow them regularly you may not miss a single new paper: but the news reaches you with a considerable time lag. Judge for yourself: the editors of the abstracts have to receive the originals, make photostatic copies of them, send out the various papers for reviewing by narrow specialists, who have plenty of their own work to do. It may be several months before the editors get the reviews back, which must then be edited and prepared for printing. The printing, too, takes more time than one would like. As a result you get to know the "news" with a de-

lay of one or two, if not three, years. If you are working in a burgeoning field alongside thousands of other people, and if you have not forged ahead of them all (which every researcher dreams of), abstracts are not all that important for you. Yet even so every month you have to look through scores of magazines in many languages.

If you have been working in your field of science for a long time and are a fairly orderly person you can manage to keep abreast of the achievements in it. Things get worse when you undertake to solve a new scientific problem. Then you must study journals of abstracts covering at least the preceding two decades, a difficult and boring task, and also go through the subject indices of abstract journals. The search for materials on a given topic is not difficult as the topic is listed under a specific heading. Matters are worse when you are looking for data that have not yet been classified under a specific chapter of your science: the question of interest may get lost in other sections of the classified index.

Many young researchers content themselves with studying magazines covering only the last five or ten years. A result of this is the appearance of papers making old "discoveries" and uselessly occupying much valuable magazine space. Examples could be cited of mathematical formulas worked out "anew" three and four times. Many researchers pursue their investigations along roads leading into blind alleys. The pity is that the uselessness of the work was revealed ten or twenty years ago, but the young

researcher knows nothing of this and wastes his energy and time.

If you follow current scientific literature haphazardly there is the danger of plodding in someone's footsteps, thereby creating uselessly repeating studies being carried out not only in other countries but even within your own country.

Since the number of scientific publications is increasing steadily, the difficulties and drawbacks just mentioned are multiplying at a high rate. They sensitively reduce the effectiveness of research work, and it is high time to consider the ways and means of overcoming these shortcomings of growth.

It is simply impossible to get along without a knowledge of English. Since World War II, English has decisively replaced German as the principal language of science. At international science forums more than nine-tenths of all the papers are delivered in English. Although many Western journals accept articles in any of the major European languages, authors, nevertheless, usually submit their papers in English as they can then be sure that more people will read them.

It is gratifying to note that the share of scientific "output" in Russian is substantial. Since the war we are being translated a lot and many people abroad are studying Russian.

Naturally, I must read much myself and make sure that my colleagues are keeping abreast of world events. Still, sometimes... Sometimes it's worth refraining from reading. It's useful to think out your investigation to the end without being subjected to the hypnotism of other

people's ideas. This is especially true if you've managed to edge even just a little bit ahead. Then you should advance for a while without looking back.

Reviews and compendiums, however, are a must. There are yearbooks in which highly qualified researchers offer so to say a bird's-eye view of developments in a large domain of science over the past twelve months. I consider this to be essential reading for every researcher. Unfortunately, not many spheres of learning have such annual reviewers, though the need for them is very great, at least until such time as we shall be able to enlist the help of robots in getting scientific information.

High-speed electronic data machines are the researcher's dream. Just imagine the possibilities! You step into the robot's room, press a button and say into a microphone:

"Could you please tell me about all investigations of the physico-chemical properties of naphthalene crystals in the last ten years?"

A moment later a mechanical voice replies:

"Please note: measurement of heat of sublimation — *Journal of Physical Chemistry*, 1958, page 125; measurement of heat conductivity — *American Journal of Chemical Physics*, 1961, page 327..."

A dream? Yes, still a dream, but a feasible one. In future information machines will replace journals of abstracts and radically facilitate the researcher's quest for past data and monthly or annual news.

Such machines have become a necessity. Without them science would grind to a halt within

several decades: the rate of scientific progress is steadily expanding, and in the near future, instead of today's million papers a year, we shall be inundated with tens of millions.

Very often tens and even hundreds of laboratories scattered all over the globe are simultaneously engaged in tackling the same problems. It wouldn't be bad if the work of all these researchers followed a unified plan. There is no such general plan, however, and duplication is inevitable.

Unfortunately, our world is divided into two camps. Capitalist states feverishly building up stockpiles of armaments allocate large sums for scientific research directly or indirectly connected with the development of new means of destruction. The countries of the socialist camp are also forced to channel applied science for the solving of military problems. Works of this kind are classified, "closed", and no exchanges of information are possible.

This state of affairs indirectly affects natural science, insofar as it is impossible to predict the practical importance of a research. Hence coordination of science on a worldwide scale is impossible.

The existence of national frontiers is another great obstacle in the way of the development of science and a source of senseless waste of energy of a vast number of scientists working independently of their counterparts abroad. It is hard even to imagine the qualitative leap that will take place in the rate of scientific advance when the world becomes united.

Meanwhile, to be sure, there is plenty to do to make order in our own house. The number of scientific investigations in our country is multiplying with each passing year and the coordination of work on a national scale has become an absolute necessity. In this we are entitled to look forward to swift and complete success.

Conferences play a great part in achieving coordinated work. How are they organized?

Usually they are sponsored by the central scientific organizations, whose tasks include the planning of meetings of researchers. An organizational committee is set up which decides where and when a conference should be held. Hundreds of invitations are sent out to organizations interested in the topics to be discussed, together with requests to submit applications and abstracts of papers. Depending on the number of papers, a conference may last from two to ten days. Papers on general topics are read before all the participants, those dealing with specific problems are presented at meetings of sections.

My own experience suggests that the most successful conferences are usually those that are held in cities like Krasnoyarsk, for instance: far away from the "centre". The travel expenses are certain to pay off.

A chance to travel to the banks of the Yenisei, which may never occur again, attracts many busy people engrossed in their work who otherwise feel reluctant to leave their laboratories. That is why choosing an exotic place is not a bad means of ensuring a conference's success: the greater the number of leading scientific lights

attending it the more it can achieve. Secondly, such a conference is a tremendous stimulant for the local scientists working in the fields to be discussed at it. Naturally, the choice of venue should not be fortuitous, and Krasnoyarsk, say, is suitable only if the subject-matter is studied locally. Every conference "on location" gives tremendous impetus to the development of science there, a very important consideration.

So it is quite normal for a scientist to travel several times a year to Krasnoyarsk or Tartu, Odessa or Kishinev, where he can meet all his colleagues in the field and discuss things with them.

The reader may wonder what I mean by "discussing things with colleagues". After all, a conference is convened to hear the papers presented. That is certainly so. Without papers there is no conference. But if not for the possibility of meeting colleagues, if not for the temptation of unhurried discussions with like-minded people, if not for the desire to pick a fight with a scientific opponent, expose his insolvency and show your own superiority for all to see — without all this conferences would be utterly shorn of their attraction, and their usefulness would be reduced to a minimum.

I had felt for some time that scientific papers did not yet constitute a conference, just as vegetables do not yet constitute vegetable stew; however I forebore from expressing these subversive views until I attended my first international congress. It was in 1956: before that visits abroad were for the few. The conference was in Montreal, one of Canada's best cities.

A quarter of an hour after landing there I was sitting in an American car next to the driver, my professional colleague. The transition from the relaxed atmosphere of the airliner to the whirlwind road traffic of the New World was rather too sudden. I had not yet been able to appreciate the quality of the brakes and other advantages of high-powered American cars, and several times in the course of the half-hour drive from the airdrome to the university campus where the congress was being held I couldn't help squeezing my eyes tight: an accident in which you are one of the chief participants is hardly a pleasing sight.

Nevertheless, my companion's lively banter didn't prevent him from delivering me in one piece to the university's student hostel. Spurred by impatience to be at the hub of events as quickly as possible, within a quarter of an hour I entered the room where a paper was being read. (I was late for the opening of the conference, of course. I forgot to mention this, because, alas, it had become a national trait for Russian scientists to be late to international meetings.) There were some 200 people in the room (there were supposed to be 800 participants, the thought crossed my mind; where were they?), the lights were out and the speaker was displaying diagrams of his experiments with the help of an epidiascope. I found a seat, but soon realized that I was unable to concentrate on my foreign colleague's speech. In the last few hours I had got used to a rapid succession of impressions, adjusted to the rhythm, and couldn't sit still. I left the room, walked down the stairs and out into the university campus.

There, sitting in small groups or pairs under the trees, on the grass, on folding chairs and overturned orange crates or on the steps of a flight of stairs, were the six hundred people missing in the hall. They were all engaged in lively conversations. I quickly appreciated the pleasure and value of such unconstrained discussions.

I also found that, far from being improper, it was natural for one to drift from group to group, listen in to fragments of conversation and join it if it seemed interesting. To facilitate the task of locating one another, each participant in the conference had a badge pinned to his breast stating his name and nationality. I got a kick out of correlating the looks of a person with a famous name. It was curious to see that Zachariasen looked so young while I had imagined him to be an old man. Wilson turned out to be burly red-head. But who is that tall man with the good-natured, kindly look? You come up closer and see that it's Harker. A remarkably pleasurable occupation. A pity that the first time can never repeat itself.

In the following days, and at other big conferences, I saw that most of the participants took the same attitude as I. Of course, you attend a (small) percentage of the reports that interest you, but most of the time is spent talking with your professional colleagues and taking the opportunity to verify your scientific ideas, voice your opinions, advocate your views, find out the details of your faraway counterparts' work.

Scientific meetings of all kinds and at every level are necessary and useful, and I fail to under-

stand those few scholars who put on airs and complain that "conferences take up all their time". Time from what? A discussion of science with a clever person! But that is an important element of scientific work!

Chapter 5

THE DOORS TO SCIENCE

...on reading this chapter parents of grown-up children should be able to decide once and for all whether their offsprings should enter a post-graduate course and prepare themselves for a scientific future or not.

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Molecular genetics is making such terrific headway nowadays, that one may well dream of the time — truly an excellent topic for a science-fiction story — when people will be able, by merely inspecting a cell of tissue picked off a young man under a supermicroscope, to establish the nature of his inclinations and abilities and thus decide what education will suit him best.

Education is exceedingly important. But, in shaping a person one may act contrary to his inborn characteristics (it is like teaching a horse to walk on its hind legs: the instructor spends a tremendous amount of energy, but the enjoyment the animal derives from this unnatural ability is extremely questionable); or one may foster his natural inclinations: in this case both instructor and pupil will derive enjoyment and satisfaction, and the results will be of use to society.

Inborn inclinations are widely diversified. Today in deciding upon the best future for

a young man we can do no more than watch his behaviour attentively. Not unuseful, perhaps, are psychological tests, which are for some reason looked down upon in our country.

"My boy will surely be a scientist," a mother declares. "You simply can't tear him away from books."

This is a superficial conclusion: an addiction to reading means nothing in itself.

"*My* boy," says another mother, "is a good mixer. He can't stand being alone, he's always with friends."

Well, some conclusions *can* be drawn from this observation. Perhaps one could even use it as a starting point. I make bold to try and delineate the traits and natural inclinations which I think important as raw material from which to produce a scientist.

I am not an expert in psychology and perhaps my remarks may appear dilettantish. So, firstly, an important quality, I think, is for the boy or girl to like to remain alone with his or her thoughts. Naturally, a child is a child, and any future scientist must also be able to take delight in games and sport and dances, the same as future engineers or airmen. But still, the child we are concerned with should like to remain alone, with a book, perhaps, or with a damaged wireless set, or just like that. With no nothing. Alone with his thoughts.

At the same time, such a disposition means nothing in itself. It may well be the trait of a future good-for-nothing. As mathematicians say, this is a necessary, but not sufficient, condition.

A second important quality is curiosity. All children of preschool age are naturally curious. Their endless "whys" are no more than an expression of the curiosity essential to every person to learn to live in our world. As often as not, however, this curiosity skims over the surface and vanishes as soon as the child learns to interact painlessly with the surrounding world. But if the curiosity remains and continues to develop, then this is an important sign which should not be overlooked. It is especially good when the curiosity is persistent. The desire to get an answer to a question should be stubborn and insistent: if the parents are unable to provide it, there are neighbours; if no one can explain it, there are books; if the right books are unavailable — I can figure it out, verify and test it myself.

These two qualities are sufficient to risk planning an academic future. But in what field?

No tests, I think, can say what it is better to be: a chemist or a biologist, a geologist or a hydrologist, a lawyer or a historian. The choice of one's narrow profession is a work of chance. Still, a kind of general classification based on natural bent can be undertaken. The curiosity is there: what is it aimed at? Interest in human destinies, in relationships among people, in man's position in society, in the spiritual life of those about us — if it leads a young man to science at all, will most likely lead him to the humanities. Interest in how things are made and how they serve people will lead him into the camp of applied scientists. Interest in nature

will direct his academic inclinations to the natural sciences.

Sometimes parents doubt whether their son should launch a scientific career. His abilities are average, he studies so-so; but, on the other hand, he has the desire. In this case every effort should be taken to coax the youth to science. Abilities may display themselves later, but even if they are not above average, devotion to one's work will surely lead a person to his place in science, and the joy he derives from scientific creativity may be a hundred-fold greater than his actual contribution to science.

Traits useful for a good scientist — a clear and logical mind, a good memory — may fail to reveal themselves if a child studies in a poor school and, furthermore, the thought of a scientific career doesn't occur to his parents. In such circumstances even an exceptional combination of such traits may prove useless. This is most regrettable, and this is why the work our Siberian mathematicians have undertaken with the purpose of seeking out the most talented young people and drawing them into science deserves the highest praise.

On the other hand, if a child is brought up in an academic family, and if, furthermore, his friends come from a similar circle, his road into science is usually preordained. True, very often, unfortunately, without any real grounds for this.

The influx of young people into our institutions of higher learning is tremendous, and it is mainly there that their destinies are decided. Education by word and by example is extremely

important. A poor lecturer or one indifferent to science may scare a student away from science. On the contrary, an inspired teacher and intensive scientific work carried out at the college or university facilitate the blossoming of natural scientific abilities and instill love for science.

Then the time comes for taking a decision and the young man says, "Science will be my profession." His decision alone, however, is not sufficient to ensure the success of an academic career. He may join a post-graduate course or the staff of a research establishment, or he may be frustrated by the need to go to work at an industrial enterprise, a hospital or a school.

Let us consider the straightest road to science: a post-graduate course.

You get a telephone call from the board:

"Will you be taking on post-graduate students this year?"

"Yes."

"How many people?"

I ponder: two? No, perhaps three.

Don't imagine that I like the idea of having more pupils. Working with a post-graduate student may turn out an extremely ungrateful task. Besides, there is the responsibility. Taking a post-graduate student means that you undertake to turn out a new scientist within three years. If you don't — the fault is yours. If your student proves lacking, you shouldn't have taken him in the first place. If you can't say anything bad about him, the fault is all your own: you organized his work poorly, you didn't provide him with the necessary equipment and, the gravest accusation, you failed to provide

your student with a "dissertationable" (don't look for this word in a dictionary, you won't find it) theme. Giving a student a theme from which he can't make a thesis is like placing a child at the centre of a maze: he is hardly likely ever to find his way out.

So it would appear that the less post-graduate students you have, the better. However, one must always reckon with the scientist's grasping nature. Just as a jolly, happy-go-lucky family never has enough money, no matter how much its wages rise, so in a good research laboratory there is never enough room, equipment or, especially, personnel. You have a flash of inspiration that must be verified, but there is no one to do it as everyone is engaged in just as interesting work. It is almost useless to request an enlargement of the staff, and in any case it is a bother and a trouble, much worse than fussing with a post-graduate student.

So you stake a claim for as many post-graduate students as you have place for tables shoved up end to end.

Then the young people begin calling.

"How did you know I was taking post-graduate students?"

"I've long been wanting to work in your laboratory and have been following the work (this, of course, is a brazen lie), and now I've heard from Nina (one of my post-graduate students) that you have vacancies..."

"Hm... When did you graduate?"

"This year."

"Will your department give you a testimonial?"

"I suppose so, I wasn't a bad student."

"Why didn't you take a post-graduate course at the department where you studied?"

There are two possible replies to this:

"There were only two vacancies, and the students with the highest grades were accepted."

Or:

"I don't feel like working in the field being developed at the department."

I hope you can guess what answer carries more weight. I must say that the research laboratories working under the departments of our leading universities are in the best position: they can choose the best students.

Some young people come after working a year or two since graduation. I ask each one what he has read, whether he realizes that he must have a fluent knowledge of English and that a post-graduate course is not a pleasure trip but study and work fourteen hours a day.

"Furthermore," I warn him, "forget about holidays for all three years. I may give you a fortnight at most, if your work progresses well."

My purpose is to frighten them away with difficulties. The weak-hearted should drop away at once.

"Well," I conclude the conversation, "hand in your papers and prepare for your examinations."

I must make a confession. It would be all very well if the number of applicants exceeded the number of vacancies. Often it doesn't. But you need workers in the laboratory. In this case you screen the applicants with half-closed eyes.

Our nursery books teach us that no good ever resulted from greed. Sometimes you get a weak student incapable of doing better than a laboratory assistant working under a researcher. You manage to teach him the techniques of the job but you can't make a self-reliant scientist of him. He leaves the laboratory at the end of his course in much the same status as he arrived. If a scientific leader is a sensitive man he feels himself to blame for dragging the young man into a job far above him — and writes the dissertation for him.

Such cases, however, are rare.

If a post-graduate course has failed to develop a love for science, the young candidate's academic career is as good as over. One can hardly hope to leave a trace in science without loving it. The man will stick out like a white crow until he finally decides to find a job in which he will be an equal among his colleagues. One need not shed tears over the failure, though. Many people take up post-graduate courses. At the end, those devoted to science remain in it, the others return to industry or take up teaching. And this is very well too, for the three years spent on the course tells favourably on their work. Thus, science, industry and education all benefit from the system of post-graduate studies.

One doesn't have to take a post-graduate course to prepare a thesis; I have met many people whose accomplishments merit the highest esteem.

After finishing college a young man goes to work at a factory laboratory or an industrial research institute engaged in carrying out urgent government assignments. His work is important

and difficult and takes up all his time. It cannot be made the subject of a dissertation, though, as it lacks an essential element: the discovery of some new scientific fact. It runs into snags. Working along prescribed lines, the researcher encounters contradictions, irrelevancies, and perhaps winds up in a blind alley. He forms new ideas about how the work should be done and starts experimenting and testing. The work is not part of the official plan and it must be carried out late at night. He has to read a lot and hunt for relevant material in books and magazines.

A capable and persistent man is bound to succeed. His work has genuine scientific value and can serve as the basis for a dissertation. It goes without saying that such a work is especially valuable and is worth much more than that of a post-graduate student holding on to the skirts of his scientific leader.

Presentation and "defence" of the dissertation is either the upshot of an academic course or a necessary milestone on the road to science.

The man's subsequent career depends on his abilities and temperament. There is room enough for all, for the conscientious plodder and the blazer of new trails, for the bustling and the cool, for the ambitious and unambitious.

There are, of course, many workers of all ranks and grades who take their jobs seriously but do not really feel themselves members of the scientific community. It is not of these that I speak, but of those to whom science is their life-work.

Devotion to science pays off abundantly. Life is full-blooded and interesting. Every day

brings something new and you live in an eternal fervour of expectation. Will you manage to complete your computations? What figure will an experiment yield? Will your experiment bear out your theoretical premises?

Research work is infinitely exciting. You encounter something strange and vague. Your experiment yields an incomprehensible result. You go through all possible explanations in vain. The mystery is with you all the time, it worries you at work, at home, in the underground train. Your mind fingers, inspects, envelopes the strange phenomenon from all sides. You seek approaches to the problem more stubbornly than a mountain-climber seeks the road to an unaccessible summit. Then something like the truth flashes in the darkness and you begin to see a line of reasoning; very slowly the light dawns and the road opens up before you. Now you can take paper and pencil and try to advance with the help of mathematical formulas or logical reasoning. You are unable to tear yourself from the work until you have reached the end.

Logic fails to solve the mystery. Hence the initial premises must be wrong. You start all over again. Days and weeks and months pass — at last there is victory. All the pieces fit together in an ordered pattern. The mystery has been cleared, the phenomenon explained. You experience a euphoria of joy and satisfaction. You feel an irresistible urge to broadcast your achievement to the world, to share your success and see its importance and usefulness recognized.

Since a researcher derives tremendous satisfaction from merely speaking of his work, he readily

accepts every proposal to do so. He will travel any distance heedless of fatigue or business.

It is natural for a person to talk about himself and his work, even though only a small circle of people may show interest. A researcher, however, is in love not only with his work, but with his profession as well. He derives as much pleasure from explaining any problem relevant to his field, not just those he is working on. Never be afraid of interrupting him in his work with a meaningful question; his answer will be comprehensive and exhaustive, and after the interview you will realize that the researcher himself is satisfied with the conversation, with being able to be of help with his knowledge and experience. Such unselfish assistance comes as natural to a scientist as breathing, for it involves his work, the work of his life, and that is why it is wrong to fear that he may be sorry for the time wasted.

It's quite another thing, though, when the wife wants to get her academic husband out to the theatre or to a beloved aunt's birthday party. He can never explain that his work is a hundred times more interesting than drinking vodka or discussing the latest Moscow theatrical news. Still, having recalled that all last week he never got home before 10 p. m., the scientist takes pity, gives in with a heavy sigh and goes visiting. Even then his troubles are not over.

"Dear," the wife plucks at her husband's sleeve, "Anna Ivanovna is asking you for the second time whether you liked the production of *Hamlet* at Okhlopkov's theatre."

"*Hamlet*? Oh, yes... Yes, of course, marvellous."

He returns with a start to the strange world in which people are interested in trifles and keep him from pondering on the unusual behaviour of dichlorobenzene in transition from one phase to another.

It is not every company that derives satisfaction from the presence of an academic. He is, in fact, hardly there at all: in the flesh he may be visiting, but his mind is in the laboratory. Not that scientists are always dull guests. Far from it: even a scientist's work has its natural intervals. That is the time for him to joke, dance, drink vodka.

The life of a devotee of science is not alien to the pangs and joys of ambition. Someone has been working on the same subject as you. He has obtained more accurate results and published them before you. Your work is now useless. It is impossible to describe how painfully one suffers — and what joy one experiences if the opposite is the case.

Your work to which you have devoted so much thought and energy has been published. But your colleagues don't mention it. No one seems to have noticed it. A very unpleasant and gnawing state of affairs. But then, two or three years later more and more references to the work begin to appear; now you know that people are reading it and using it, which means that it has contributed something to the advance of the whole scientific front. You are filled with tremendous joy, with a deep sense of satisfaction, with a realization of your usefulness, and if the word "happiness" has any meaning, this is it.

Chapter 6

SOME HISTORY

...which tells how the study of nature by the method of verbal juggling was replaced by experiment. The reader will also learn that the wonderful achievements of science made the physicists of the 19th century overconfident: they thought that all that was left to us was to reap the fruits of their work.

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In its development the human embryo repeats the whole cycle of evolution that, over millions of years, transformed a tadpole into the king of nature. It seems natural, therefore, to try and compare the evolution of ideas from the ancient Greeks to our time with the development of the conception of the world in a contemporary child.

However, we are forced to abandon the intention almost at once. Among the sages of ancient Greece we find Aristotle and Democritus. The simple, clear reasoning of Democritus is close in spirit to contemporary thinking. Aristotle is another matter. The trust in the inner meaning of words on which his reasoning is based is, to a degree, characteristic of the naïve mentality of a child. Today Aristotle's *Physics* is no more than quaint reading; Democritus' ideas on the structure of the Universe, on the other hand, could, with some editing, be used as a mo-

dern popular exposition of the fundaments of science.

Christianity chose to embrace Aristotle's hodgepodge of naiveté and mysticism. Democritus' atomistic teaching, on the other hand, clearly led to the rejection of God. Accordingly, towards the end of the 13th century Aristotle was proclaimed Christ's forerunner in the explanation of nature. Up to the 17th century any criticism of Aristotle was regarded as subversive to the tenets of the fathers of the church. A decision of parliament in Paris dated 24 August 1624 forbids, under penalty of death, any person "to hold or, especially, expound any truths contradictory to the teaching of Aristotle".

The indignantly denounced godless ideas of Democritus would probably have been lost for posterity if the Roman poet Titus Lucretius Carus, more popularly known by his middle name, had not chosen them as the theme of his poem *De rerum natura* ("On the Nature of Things"). I advise you to read this excellent book, if you have not done so already. In spite of its naiveté, the poet's sincere admiration of the simple and clear atomistic theory which developed a harmonious system out of diverse and sundry observations of the world, is conveyed to the reader.

If we wish to gain an idea of the mentality of scholars of antiquity and the Middle Ages, we must read the works of Aristotle.

As we wade with difficulty through the verbal maze, we can ultimately discover the principle on which Aristotle bases his explanation of natural phenomena. Whereas the ancient atomists, like present-day physicists, assumed

that nature should be explained in terms of quantitative categories — spatial extension, geometric form, motions of bodies and particles — Aristotle "explained" nature by ascribing every property a mystical attribute, which explains things on the level of mentality of a five-year-old child. Why is a thing sweet? Because it contains sweetness. Why is a thing warm? Because it contains heat. Everything is explained with the greatest ease.

Why do bodies fall to the gound? Very simple: they are borne down by their inherent ponderability. The more ponderous a body the faster it falls. The idea that the earth acts on a falling stone is utterly unacceptable to Aristotle. A body's behaviour is determined by its "nature", by its inherent properties.

The possibility of providing a "simple" explanation for anything on earth is remarkable: verbal juggling is brought to perfection. The atomists accepted as axiomatic (just as contemporary physics does) that the particles of matter are in eternal motion; Aristotle's physics proceeded from the premise that every motion must have a motive source. The motor may be inside the body or next to and touching it. Action at a distance was considered quite impossible. Suppose you accept the basic premises, how then do you go about explaining, say, the motion of a thrown stone? There is no motor inside the stone, there is no pushing or pulling body. A predicament, one would think, but Aristotle is ready with an explanation: at the moment of throwing the hand imparts motion, not only to the stone, but to the surrounding medium

as well. So what? The hand also transmits to the surrounding medium (or rather, the part of it that comes into motion) a special quality: *virtus movens*, which is the ability to transmit motion to other bodies. See how simple it is?

Now everything is easy. The *virtus movens* causes the stone to move to a neighbouring position; the stone displaces a new section of the medium and transfers a little more *virtus* to it. And so on. But the stone will ultimately fall to the ground. So what? It is obvious that in each successive transmission the amount of *virtus* steadily decreases.

But what is this medium of which we are speaking? The air evidently. What if there is no air? The medium is there anyhow, for Aristotle flatly rejects the possibility of vacuum. He is equally opposed to Democritus' atoms and the concept of vacuum.

His arguments against vacuum are very spirited, and his logic can be judged from the following "reasoning": vacuum is space without the bodies filling it. But such a declaration is as logically senseless as a drink which can't be drunk or a feeling which can't be felt.

I offer no further examples. This is sufficient to form an idea of the nature of Aristotelean scientific reasoning.

I have had occasion to read manuscripts written by contemporaries in the spirit of Aristotle. When an ignorant person undertakes to write about science this is just about what he produces.

Ever since antiquity and up to our time verbal play has been a tool of religion; it is infinitely alien to scientific knowledge. It is, of

course, far from accidental that the Franciscan and Dominican monks — the most intolerant of all Christians — found support in Aristotelean science. Several theologists made a successful synthesis of vague Aristotelean phrases and religious dogmas; most noted among them was Thomas Aquinas.

In the course of 200 years the church carried out some work extremely useful for itself. Subsequently, however, it proved its undoing. Contemporary Christian philosophy traces its roots to Thomas Aquinas. These roots are closely intertwined with Aristotle's teaching of nature. But when experimental natural science appeared on the scene it became impossible to uphold Aristotelean science. Religion was compelled to part ways with Aristotle, suffering ideological losses in the process.

The new period in science began in the 16th century. It was heralded by the discoveries of Copernicus and the works of Pierre Gassendi, who revived the atomistic theory of Democritus.

Observation and experimental investigation began to replace scholastic discourses about nature. It became clear that words serve to denote phenomena, that taken by themselves they are incapable of explaining anything. This change of accent is especially apparent to the historian perusing the works of the great Italian, Galileo Galilei, founder of experimental physics. It is not known for sure whether Galileo staged any experiments to verify his assertions; the important thing is that he described the experiments that could be carried out to this purpose.

Galileo's approach to problems was like that of contemporary natural sciences: before explaining a phenomenon, it must be described.

He deliberately left aside the question of why this or that type of motion takes place. He is concerned with the question: How does it take place? And the question is not of explaining, but of describing it. This restriction, which Galileo imposed on himself, was of a temporary nature. It was apparent to him that the question of the causes of motion can be posed only after all the facts were described.

As for playing with words, Galileo had no doubts as to the futility of this as a method of explanation. Here is a remarkable excerpt from the famous dialogues between Salviatus (through whom Galileo speaks) and Simplicius (representing the Aristotelean school). To expose his opponent Salviatus asks:

"Why do bodies gravitate towards the earth?"

"Everyone knows that the cause lies in the Ponderability of bodies," Simplicius replies.

"You are wrong, signor Simplicius. You should have said: everyone knows that the cause is called gravity."

Salviatus then goes on to explain that, by giving a name to a phenomenon, we have not advanced one step towards its understanding. He concludes with the admonition: do not play with words.

Thus, the verbal castle built by Aristotle was reduced to rubble.

The new edifice of science began to rise in its place; it started with mechanics. In 1687, the work of the great English physicist Sir Isaac

Newton, *The Mathematical Principles of Natural Philosophy*, appeared. It laid down the fundamental laws of motion for all bodies.

All? The future revealed the need for certain reservations. But for 200 years before that numerous proofs testified to the remarkable accuracy of Newton's laws. Never was there any thought of challenging the absolute validity of Newtonian mechanics; on the contrary, confidence in its virtually divine nature began to develop.

The discovery of the laws of mechanics was followed by some remarkable mathematical investigations that were immediately used to solve problems of mechanics. The new problems of mechanics, in turn, posed new tasks before mathematics. Not much time passed before the researchers were prepared to answer the question how a body would move. All they required was knowledge of the initial conditions: the body's position and velocity at a given moment. Its subsequent fate was in the scientists' hands: the laws of Newton in the form of differential equations described it completely. The laws stated whether the body would travel along an ellipse, a parabola, or some other curve. If one wanted to know the velocity of motion all one had to state was the time and point — and Newton's equations would provide the answer, just as they would to any other question concerning the motion of the material particle that interests us.

True, there is a little "but". In order to forecast the future we need information concerning the field of force in which a body moves. The

great Newton, however, enunciated not only laws of motion. He also provided us with the famous gravitational field formula, a beautiful and simple formula that makes it possible to calculate the forces of interaction of two bodies if we know their masses and the distance between them.

It is understandable, then, why one of the primary applications of the whole wealth of mechanical and mathematical ideas is to the motions of heavenly bodies. Such remarkable successes were achieved in explaining the behaviour of planets that it became hard to be pessimistic and question the universal validity of Newton's laws. The crowning achievement was, of course, the calculations of Urbain Leverrier. The story has been told in countless books on popular science. Still the example is so fine that the author is unable to resist the temptation of telling the story again in the hope that a substantial number of his readers may get the know of Leverrier from his account. By the year 1845 the motions of all the planets had been calculated and the calculations agreed excellently with astronomical observations. The planets all arrived unfailingly at the points of the sky ascribed to them by the calculations. All? No, not all. Uranus misbehaved itself and refused to obey Newton's laws.

But this was impossible! Confidence in the unshakable validity of these laws was so great, they had been confirmed so many times, that to challenge them was tantamount to challenging science as a whole. But there had to be some explanation of Uranus' behaviour.

Perhaps, Leverrier reasoned, there is some unknown planet beyond Uranus? Its gravitational attraction did not enter the calculations, and it was too small to affect more distant planets. However, assuming it to be a neighbour of Uranus, one could explain why the latter didn't follow its prescribed course.

If this was the case, then a reciprocal problem could be formulated, and one could calculate how far away Uranus wandered from the path prescribed by the differential equations. At such and such points it deviates to the left, at others to the right, and at some points the deviation is negligible. From these deviations it should be possible to calculate how the unknown planet moves. When it is close to Uranus it acts stronger, as the distance increases its pull decreases. Leverrier carried out the relevant calculations. He determined the unknown planet's path and stated when, and at what points of the sky, it should be sought. In September 1846, the new planet was discovered at its assigned place, and Neptune joined the family of planets.

I tingle with excitement when I simply describe this remarkable example of scientific foresight: I can imagine the agitation of Leverrier's contemporaries when the news that his planet had been observed was broadcast to the world. As for Leverrier himself — here my imagination is powerless.

It is not hard to understand the boundless faith in the correctness of Newton's laws of mechanics after such an achievement.

But mechanics is only a small section of science. Is it not premature to take pride in the achie-

vements of all natural science? There are so many phenomena in other fields: optical, electrical, magnetic, etc., yet, the laws of mechanics are set above all! This was the reasoning of the overwhelming majority of natural scientists. Various phenomena differ only in the shape of their fields of force. Already Newton offered a good classification of forces, listing, in addition to gravity, magnetic, electrical, optical, chemical and cohesive forces. The task was thus to discover the laws governing the respective fields of force. If they were known, then Newton's laws would again make it possible to determine a body's fate, just as they can be used to forecast a planet's behaviour under gravity.

But what about the nature of the forces concerned?

Strangely enough, the question did not worry many people. The absence of such interest was perhaps to some extent due to the relationships that existed between science and religion. As a matter of fact, left without an answer, the question leaves open the possibility of including God in the scheme of things. Some mechanics (Maupertuis) even sought to prove the existence of God on the basis of the mathematical expressiveness of the fundamental principles of mechanics. Others declared that the hypothesis of God's existence adds nothing to science and contributes nothing to our understanding of the nature of forces (Laplace).

Other researchers, again, were concerned with making order in the kingdom of forces and reducing them all to a single cause.

To reflect on the nature of forces means to

ponder over the structure of matter. Democritus' world made up of particles and vacuum found many adherents. Newtonian mechanics made it possible to replace the unsophisticated little hooks joining atoms together with gravity forces acting at a distance. The atom appeared in works of the time as a spherical body. It was assumed that the interactions of these invisible spheres in some way explained the properties of substances.

Then, in the beginning of the 17th century, the great Descartes put forward a new theory of the universe, based on the concept of the invisible, all-pervasive ether. There is no vaccum in the universe and everything is pervaded with this propagating medium — or media, as the possibility was discussed of various phenomena having their specific ethers: an electrical ether for electrical phenomena, a luminiferous ether for optical, etc.

The ether hypothesis explained the action of bodies at a distance. Gravity and electricity perform beautifully in vacuum, in the absence of any medium. This is hard to believe, especially if, as was shown in the 19th century, electromagnetic forces do not propagate instantaneously: one body senses the approach of another not at once but with a time lag. The action, evidently, propagated through something, through some material medium.

The electromagnetic field theory enunciated by Michael Faraday rested on a firm belief in the existence of the ether. Although the ether did not in any way enter the formulas describing the behaviour of electrical, magnetic or optical

fields, it seemed impossible to do without it, and researchers didn't doubt its existence for one moment.

What was the ether? Perhaps it was a peculiar fluid in turbulent motion; or perhaps it was a quiescent fluid in which denser spheres pulsated. To explain light phenomena it was found necessary to assume that the ether possessed solid-body properties, since it propagated transverse waves.

Although no beautiful universal model of the ether capable of explaining all physical phenomena was created, and although the interaction of the ether with atoms and molecules remained completely unclear, confidence in the existence of such a universal medium was fairly firm.

The close of the 19th century saw the fine investigations of Clausius, Boltzmann and Gibbs which demonstrated that application of the laws of mechanics and probability theory to the behaviour of large assemblies of molecules (especially those of gases) provided an excellent explanation of the physical properties of bodies. These works reinforced the belief that the world rested on three whales: the three laws of Newtonian mechanics, which governed the motions of invisible particles with the same validity and precision as those of celestial bodies.

The absence at the time of any hope of studying the world on a submicroscopic scale pushed problems of the structure of matter aside. To a degree they were regarded as philosophical, metaphysical and outside the scope of natural science, as claimed by purblind philosophers like Mach and Ostwald, who insisted that phy-

sics need not concern itself with questions of structure. Perhaps they didn't regard the absence of knowledge concerning the ether, molecules or the nature of forces as a drawback to physics.

The picture of the world was there: bodies and particles moved according to the laws of Newton. The formulas of the forces representing them in terms of the properties of interacting bodies and the distances between them were known. All that was left was to substitute them into the differential equations, and all the problems of physics would be solved. Physics was thus regarded as a complete science.

In preparing to write this chapter I leafed through the old Russian encyclopaedia of Brockhaus and Efron. The volume containing the article "Heat" was printed not so very long ago: in 1891. The author dutily sets forth the laws of thermodynamics and the methods of measuring heat, but says only a few words about the nature of heat: "We are certain that heat is associated with certain motions of particles of matter." From the context it is apparent that the author considers the nature of motion irrelevant and immaterial to physics.

This was the stand taken by many theoretical physicists of the late 19th century, who declared that, as a science, physics was complete. It was this view which prompted the teacher of Max Planck to advise him not to take up physics.

"Everything has already been done in physics," he declared. "Better take up something else."

Planck refused to follow this advice, and in several years carried out his famous investiga-

tion of radiation quanta which formed the basis of modern physics.

Lord Kelvin, a talented and clever physicist, declared in one of his lectures that theoretical physics represented a graceful, complete edifice. There were only two small clouds in the skies of physics which, he thought, would soon be cleared away, leaving nothing to do for 20th century physicists.

Do you know what those two little clouds were? To do Kelvin justice, he mentioned the two fundamental stumbling blocks that confounded physicists of the end of the 19th century. One was the constancy of the velocity of light, discovered experimentally by Michelson: out of this cloud grew the theory of relativity. The other cloud took the shape of a curve showing the dependence of radiant energy on wavelength. Theory predicted that the curve should climb infinitely as the wavelength decreased, the ultimate result being the so-called "ultraviolet catastrophe". The experimentally observed relationship, however, represented a humped curve which, after reaching a certain maximum, fell off as the wavelength decreased. Out of this cloud developed quantum physics, the credit for which goes to Max Planck. Kelvin's assessment of the situation was, thus, not so bad, after all.

I hope that now the reader will understand the dismay of leading physicists of the time when the turn of the century showered one stunning discovery after another on their heads. Their dismay was so great that some of them (Lorentz) expressed regret that they had lived to witness them.

This is a good lesson, and the history of science will remember the danger of overconfidence, when an age lays claim to the complete and ultimate knowledge of the truth.

Let us now see what happened in the 20th century.

Chapter 7

THE FIRST ATTACK ON COMMON SENSE

...in which the reader finds that the word "self-evident" should be removed from the lexicon of physics. Incidentally the author sets himself the task of explaining what "to explain" means.

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The resplendent edifice of physical science erected in the 19th century did not last for long. It collapsed in 1905, the year in which one of the most remarkable creations of human genius — the theory of relativity — was enunciated by the 25-year-old Albert Einstein.

With the help of the strictest mathematical logic, on several dozen pages he set forth the corollaries of two axioms. Corollaries so unusual that they crushed existing concepts and shattered the foundations of physics. The paper was remarkable for its profundity, for the great exactingness with which it approached every apparently self-evident proposition. It was impossible to escape the inexhorable logic of Einstein's reasoning, which led readers to paradoxical conclusions despite their inner protest, conclusions that followed inevitably from the two axioms.

The surprising thing is that neither of them was wholly unexpected to the contemporary reader. Einstein simply was the first to ponder

over the conclusions to which the axioms led when taken together.

The axioms came from different fields of physics and were brought together for the first time in Einstein's paper. What are these axioms?

The first one states:

If two observers are in uniform rectilinear motion with respect to each other, they are both in absolutely identical conditions.

Which is to say that there is no way of determining which of them is moving and which is at rest. It is meaningless to ask who is in "true" motion. There is no such thing as absolute motion. All motion is relative!

This principle of mechanics, well-known since Galileo's time, states that rest cannot be distinguished from uniform rectilinear motion. On the whole it agrees well with "common sense". Everyone knows from experience that, sitting with closed eyes in a ship or airplane, it is impossible to distinguish between rest and uniform motion.

The second axiom is rather another case. Although the postulate that *the velocity of light is always the same for different observers irrespective of their motion* was known to physicists, they regarded it as a strange experimental fact that still had to be investigated. Einstein elevated this strange postulate to the status of an axiom.

The phenomenon was discovered by Michelson and Morley in 1887. The researchers had set themselves the task of comparing the speed with which light propagates from east to west and from north to south. This is something like

comparing the results of measurements in two laboratories, one of which is moving together with our planet while the other does not take part in the earth's diurnal motion. The experiment revealed that the speed of light was in both cases the same. This result made it hard to explain the behaviour of the all-pervasive, weightless ether, through which light was supposed to propagate. Various attempts to reconcile Michelson's experiment with 19th-century physics were made unsuccessfully up to 1905, when Einstein cut the Gordian knot by elevating the inexplicable fact to the status of a principle, a basic concept. It had to be accepted as a fact of nature, that's all.

Joining of the two axioms together meant an entirely new formulation of the relativity principle. What holds for the velocity of light holds good for other manifestations of electromagnetism. Hence, Einstein declared, there is no physical experiment capable of distinguishing one system from any of the infinite number of systems moving uniformly and rectilinearly with respect to one another. All these systems are wholly equal.

What are the conclusions to which these two axioms lead? University students of the year 2965 will perhaps study these conclusions on practical experiments. The teacher will ask Mike and Pete to occupy their seats in two identical rockets, press the required buttons, and send them off on a study journey. A "simple" way of demonstrating the essence of relativity theory would be to dispatch the space vehicles in different directions along the same line; each ve-

hicle has a light source and receiver facing each other across their rockets. Before their departure Mike and Pete compare watches and adjust their radio receivers and transmitters.

The teacher gives them their first assignment: they must measure the time it takes for light to travel from one side to the other of their respective rockets. When the accelerating motors are switched off the vehicles go into uniform coasting motion and the students commence their time measurements.

"Right," they inform each other and the teacher. "Here are the results."

The figures are the same: as the vehicles are exactly the same and the experiments are identical this is only correct, since the time measured by Mike according to his clock and the time measured by Pete according to his flows identically.

The second assignment is to measure the same event, but in the other man's rocket: Mike measures the time it takes a light beam to cross Pete's ship and Pete measures a similar event in Mike's ship. Obviously, the measurements have to be carried out differently. The instants corresponding to the beginning and the end of the time interval can be reported to the other vehicle by radio. Mike measures the time lapse between the arrival of the radio signals sent out by Pete and Pete measures the time interval between the radio signals sent by Mike; Mike thus measures the time according to Pete's clock, and Pete measures the time that passes by Mike's clock. The result, although differing from the initial figure, is still the same for both rockets (the con-

ditions of the measurement remain completely symmetrical): the figures are somewhat greater than in the first measurement. The students find that to a moving observer the duration of an event appears longer than it does to an observer at rest with respect to an event; and the time appears the greater the faster he is moving.

Time turns out to be relative. The results of time measurement depend on the motion of an observer with respect to the measured event.

"This result follows directly from Einstein's axioms," Mike explains to Pete (or Pete to Mike: we shall let them maintain their symmetry). "Note that the beam of light crosses the room perpendicularly only when we are observing our own respective beams."

"When we observe the other person's vehicle we each note that the beam has travelled a greater distance from the source to the receiver, a distance equal to the hypotenuse of a certain triangle one side of which is the path travelled by the other person's vehicle in the time it took the light beam to reach the receiver, and the other side of which is the width of the room. But the speed of light is the same to both of us, while the paths travelled by the beams differ. Hence, the time it took for the light beam to travel the distance from source to receiver is less for the observer inside his own vehicle (the beam travelled a shorter distance straight across the room) than for the outside observer (to whom it has appeared to have travelled along the hypotenuse). Which is just what we have observed," concludes Mike (or Pete).

With this observation they calmly have their lunch, quite unexcited by the revolutionary nature of the finding.

It was much harder for Einstein's contemporaries to accept this conclusion.

"Time is not absolute!"

"Time depends on an observer's motion!"

"The same event takes place with different speed if observed from different points!"

These statements of one and the same fact seemed strange, unusual, contrary to common sense. But it was especially hard to accept the corollary deriving from the symmetry of observers: that for each person the measurements of his "own" events yield the smaller figure. Thus, Mike reports to Pete, "My light signal has travelled this distance in one microsecond, and yours in 1.1 microseconds." But Pete informs Mike: "*My* light signal has travelled the distance in one microsecond, and *yours* in 1.1 microseconds." Placed in a nutshell the paradox is that Mike decides that Pete's clock is slow, while Pete decides that it's Mike's clock that's slow.

But can't we determine whose clock is in fact slow after Mike and Pete return from their study space trip? The clocks can be compared, of course, but this is not the way to check the conclusion of the special theory of relativity. The conclusion belongs only to this theory, which deals only with the case of uniform rectilinear motion. But in such motion observers can meet only once, and their clocks can be checked only by radio.

Still, what will happen if Mike's and Pete's clocks are compared on their return? Will they

show the same time or will one of them have gone ahead? To answer this question one must precisely describe the motion of both spacemen with respect to the stars.

One case is of special interest. Suppose a spaceship has taken off from somewhere and departed on a long journey, from which it one day returns. The clocks in the rocket and at the cosmodrome were checked before blast-off. They are checked again on the rocket's return. It will be found that more time has passed on the cosmodrome clock. If the journey was carried out at very high speed (approaching the speed of light), tens or even hundreds of years will have passed at the cosmodrome, compared with only a few years inside the rocket.

However, we have no time to dwell in greater detail on these curious conclusions. I should only like to stress that strict physical reasoning based on indubitable axioms has led to new views concerning such a fundamental concept as time. Time, it was found, is a relative quantity, which is to say that one and the same event is of different duration if viewed from different points of view.

Obviously, this conclusion of the relativity theory alone is sufficient to outrage "common sense". After patiently listening to me, my "common sense" joins the conversation.

"What's this nonsense about one point of view, another point of view? How much time actually did pass?"

"None: you can't put the question like that!"

"What do you mean 'You can't put the question like that'? It's nonsense!"

"No it isn't. There are many things which can't be asked. You will agree, for exampll, that it's meaningless to ask what city is nearer, Leningrad or Paris? To us Muscovites, Leningrad is closer than Paris, while to the people of Marseilles Paris is near by and Leningrad is far off."

"But that's quite another matter."

"No it isn't. It's exactly the same thing: it's impossible to answer a nonsensical question."

"But why should the question of the time lapse between a shot and the bullet hitting the target be nonsensical? After all time..."

"Yes, yes: time... You wanted to say what time is?"

"Time is... But everybody knows what time is... If you like, it's that which we measure with clocks."

"Excellent. Quite correct. This is the best possible answer. And this is what I began my explanations with. I only wish to draw your attention to the fact that we usually carry our clocks on our persons, and in this way we can easily measure our own time; as for other people's time..."

"That's nonsense. All time is the same."

"But it isn't. The first traveller has his clock, the second has his, and if they want to check them they must send signals to each other. As I told you, one person observes the time an event takes according to his clock, while the other — the moving one — sends signals telling the time his clock showed at the beginning and the end of the event. And we come to the conclusion that the interval between the events as measured by the first observer is longer."

"But you're talking about clocks, while I'm speaking of time. Time is..."

"What is time? You've just agreed that time is that which is measured by clocks."

"Please, don't confuse me. I feel that something is wrong here. My mind refuses to accept it."

Yes, it is certainly hard to fight against common sense. But it's even harder to argue with a person who throws logical reasoning aside for the benefit of accepted "truths". It goes without saying that thousands of physicists hailed the theory of relativity with amazement, wonder and reverence before the power of analytical thinking. They checked Einstein's logical reasoning and failed to find a single flaw in it.

The adherents of common sense, however, continued to express indignation and anger and demand "other proofs" for many years (it is amazing that some voices can be heard to this day). Yet there were plenty of proofs, and they appeared in abundance many years later, when physicists began working with particles travelling at speeds approaching the speed of light.

I have mentioned only the conclusion of the relativity theory dealing with time intervals. There are other revolutionary conclusions that derive as strictly from its fundamental postulates. Among them is the conclusion concerning the increase of a particle's mass with speed, and the conclusion of the equivalence of energy and mass.

It proved very simple to confirm the increase in mass experimentally, and this was done long ago for electrons. Verification of the equivalence

law became possible when physicists began to tackle nuclear transformations and Einstein's equation became the basis of all calculations of nuclear reactions. One of the latest tests was the direct verification of time dilation in the case of a moving particle in laboratory conditions.

Nowadays, to be sure, physicists (with rare exceptions) do not regard these experiments as verification of the theory: it has been accepted unconditionally and become a basis for their daily work.

The importance of the relativity theory for physics goes beyond its meaning of a new law of nature. It brought about a gradual change in the psychology of researchers working in natural science. Physicists became extremely wary of the conclusions of "common sense". They learned to investigate from every side each statement claiming to proclaim an objective truth. They became wary of words, of empty words with nothing behind them. They felt the need to remove even the slightest traces of the Aristotelean atmosphere from science.

The example of the time paradox taught physicists that every concept appearing in their equations should either answer the question, "How can it be measured?" or be connected with measurable quantities by functional dependencies.

If the way in which a quantity can be measured or calculated is stated, then there is nothing to be added. Nature is objective, which is to say that it exists independently of the researcher; physical quantities, however, are propo-

sed and introduced into common usage by the observers of nature to facilitate their description of it.

Gradually, though with an appreciable lag behind the development of science, the textbooks are rid of empty definitions, meaningless combinations of words, definitions that create the impression of some hidden meaning yet to be revealed.

"What is force?" a teacher asked.

"Force is a physical quantity measured by the degree of stretching of a spring," the pupil answered, and his answer was not at all bad.

"No, no," the teacher declared. "You have said how to measure force, I am asking what force is."

"Force is... is impetus, action, the cause of motion," the pupil mumbled, trying to recall what the textbook has to say on this score.

"Very good," the delighted teacher responded.

Actually, the first answer was good. The rest is meaningless, empty words.

After the lesson taught by the relativity theory physical construction became immeasurably clearer and stricter. Physical explanations of phenomena became clear-cut and well defined.

I am occasionally asked by the Ministry of Education to attend school examinations in physics. If I find that a pupil seems especially capable I ask the teacher's permission to ask him several questions.

"What happens to a copper rod when it is heated?"

"It expands," the pupil says, wondering whether this simple question has a catch somewhere.

"Why?"

"Because all bodies expand on heating."

"Excellent. Why?"

The pupil ponders. "On heating the atoms move faster and they as it were push farther and farther apart, the average distances between them increase, and the body's size increases."

"Excellent." Here I pause briefly. "Now tell me, why do the atoms move faster on heating?"

A bewildered silence. The pupil casts helpless glances at the teacher, and I can read the mute reproach in them: "You never told us this." The teacher is also knocked out of the saddle and thinks, "You and your questions: why do atoms move faster? Who knows why?"

But one pupil out of ten, shrugging his shoulders wonderingly, answers: "Particles move faster when heated because it's a law of nature."

Beautiful! This is just what I wanted. The lad realizes that the physical scheme of explaining phenomena consists in extending the partial to the general, in the logical demonstration that a given phenomenon is a special case of a general law of nature. And a general law of nature is, today, the ceiling of our explanations. A general law of nature is called so precisely because it can't be derived from anything. Of course, the state of affairs may be temporary; as science advances the ceiling of explanations climbs higher. What today seems to be a general law of nature may in several years turn out to be a corollary of a newly discovered still more general law of which the old law is merely a special case. This is just what happened with Newton's laws of motion. Since Einstein's discove-

ries, we regard Newton's equations as a special case of the laws of motion for low speeds.

Einstein's unparalleled accomplishment also led to a deeper understanding of the role of theory in natural science. If previously one asked a physicist what the purpose of theory is he would most likely say that it is "to elucidate the nature of a phenomenon, obtain a picture of the phenomenon, discover its mechanism, gain a graphic idea of it". I think that today few physicists would offer such a vague answer. Nowadays such a question would evoke a clearer and, if you care, prouder answer: "The purpose of theory is to predict phenomena."

The idea that notions of nature should be graphic and capable of modelling, so highly valued in the 19th century, has proved insolvent. The relativity theory offered no new mechanical model to replace the ether it buried, yet its power and value were unquestionable, it made possible the forecasting of many important phenomena even before people had the slightest idea of how they could ever be observed.

Think of this. Is it not remarkable that the human intellect removes the element of suddenness and is capable of predicting the outcome of events that have not yet taken place! Is this not the power which religion has persisted in assigning to divine force? In its urge to know the world, natural science has no other purpose than that of predicting the future.

However, it was not the relativity theory alone that built up contemporary physical thinking. An important part of this was also played by the remarkable discoveries in the world of atoms.

Chapter 8

CAPITULATION

...which tells how "common sense" was finally discredited as a result of the discovery of the law of motion of electrons.

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When a child wonders how his favourite toy works he tries to find out by taking it apart. Man evidently retains this curiosity all his life. At least that is how I explain the current general public interest in the structure of matter.

"What is a molecule made of? Oh, yes, of atoms, I recall. But how, for example, is a molecule of water constructed?"

"That's an easy question. Look at this drawing. An atom of oxygen in the middle with two atoms of hydrogen on both sides."

"Wonderful. So simple; science has even been able to establish that the three atoms do not lie on one line. Now I have an idea of the structure of a molecule of water. But what is an atom made of?"

By the beginning of the 20th century physicists accepted the model of the atom proposed by Earnest Rutherford according to which a tiny positively charged nucleus was surrounded by a swarm of electrons corresponding in number to the element's place in Mendeleev's Periodic Table.

"How simple," the readers of contemporary popular magazines exclaimed. "Just like a planetary system."

But physicists broke up the particles of matter into smaller and smaller fragments. Soon they reached the atomic nucleus, which turned out to be made of neutrons and protons.

"Remarkable," the readers said. "But is the nucleus also like a planetary system?"

"No, no," the physicists said. "You can picture the nucleus as, say, peas in a saucer. Is that clear?"

"Yes, of course, it's so simple and absolutely clear."

By gradually increasing the power of their instruments the physicists continued to knock particles against each other and study their transformations. By the middle of our century sufficient experimental data were accumulated to make possible an answer to the persistent questions of lovers of science.

"What are protons and neutrons made of?"

"It has been established," the physicists reply, "that a proton turns into a neutron and a positron."

"Very interesting. So a proton consists of a neutron and a positron?"

"One moment," the physicists say. "You can't put it that way. The thing is that experiments show that a neutron turns into a proton and an electron."

"How can that be? I fail to understand. Is a proton part of a neutron or a neutron part of a proton?"

"Neither," the physicists say. "The proton

and neutron are elementary particles, which are governed by the laws of transformation."

"Hmm... I see..." the 20th century reader mutters hesitatingly. "But I thought the particles were elementary. How can they be elementary if they change? Besides, here we have protons in neutrons and neutrons in protons... The old picture was better... More researches are necessary."

As long as structural pictures can be drawn on paper it isn't hard to grasp the essentials of physics. Sometimes, instead of a drawing on paper, a familiar image can be invoked, like peas in a saucer, or a common fact to illustrate a point. The listener is satisfied and imagines that he has understood everything. As one of our leading physicists, the late Yakov Frenkel, liked to say, "Things are not incomprehensible, they are unusual." This is the golden truth.

Towards the end of the forties I gained notoriety as a public lecturer. Time and again I was asked to explain atomic energy. After several lectures, in which I tried to link the atomic explosions with Einstein's law of equivalence, I realized that my audience had difficulty in grasping my explanations. I had to change my presentation of the subject. I tried beginning my lectures with the statement: "We all know that burning firewood yields heat." The audience nodded in acquiescence.

"The evolution of heat is the result of the chemical reaction of burning," I continued. "Molecules of oxygen collide with the molecules of the fuel, breaking up the old ones and forming new ones."

I then went on to explain that the new molecules moved faster than the old ones. That was the main thing, since heat is associated with the speed of molecular motion.

"Is it clear now why burning wood yields heat?"

The audience declared their complete understanding. Everyone has seen burning firewood, and science stated that that was how it should be. I went on.

"The liberation of atomic energy is very like the liberation of chemical energy. Only in chemical reactions collisions occur between molecules, in nuclear reactions the collisions are between atomic nuclei."

This is a round-about manoeuvre in the course of which I explain new concepts on the example of commonly known things. It succeeds invariably as the new is reduced to the habitual. In effect the explanation is not of the new phenomenon but of the habitual one, and the transfer to the new idea, coupled with the statement that the two phenomena are completely analogous, is accepted naturally.

However, we have drifted from the subject. We were saying that the gains of physicists in respect of the structure of matter could be popularized easily insofar as they agreed with common sense and could be interpreted with the help of simple drawings and models. This is confirmed by the following fact. Shortly after the war I wrote a popular booklet called *The Structure of Matter* which became something of a bestseller. The editors received many interesting letters from readers. One of them was from a collective-farm dairymaid.

"Dear professor," she wrote, "we read your book during our lunch break. It is written so clearly and simply that now we all understand how particles are constructed."

Thus, it is not hard to explain the structure of matter. It is quite another thing to present a popular exposition of the laws of motion of particles. These laws were discovered almost 40 years ago, and they reduced most contemporaries to a state of utter bewilderment. Why? Because there was no analogue that could be invoked to offer an idea of the nature of an electron's motion. There is nothing customary on which to fall back.

In our notions of the nature of motion of invisible particles of matter we try to proceed from daily experience. There are only two possible visualizations of our notions of motion. One is that of a particle moving like a tiny bead so that at every subsequent moment it is displaced from one point of space to another. We know that we can photograph this motion: the photographic plate will show the particle's track. The alternative to this visualization, for the case when the movement of individual particles cannot be seen, is the displacement of a continuous medium (waves at sea is a good example).

Up till 1925 there was no doubt that the motion of matter, whether it concerned light or radio waves or electrons, followed either the one or the other pattern. In fact, it is impossible to imagine a third mode. But then it was found that elementary particles behave in some cases like beads and in others like continuous matter. It is impossible to transfer the laws of motion

of the greater macrocosm to elementary particles.

Up till 1925 it was axiomatic that to describe a particle's motion meant stating the path it followed and its speed at every point of that path. This, however, proved impossible for the electron and other elementary particles. The fundamental law of motion of elementary particles (not universal, but embracing a very broad class of events) was enunciated by the German physicist Erwin Schrödinger. The basic premise of the new science, which came to be known as wave or quantum mechanics, was unusual. Unlike classical mechanics, knowledge of external forces is not sufficient to define a particle's path and velocity. The laws of the new mechanics can be used to calculate only the probability of a particle's position.

At first glance it might seem that, far from being a revolutionary breakthrough, wave mechanics is but a poor theory incapable of calculating the mechanical motion of an electron with sufficient accuracy. This, of course, is not the case.

It was later shown that Schrödinger's equation provides adequate knowledge of an electron's behaviour, and the data which cannot, in principle, be calculated, cannot, in principle, be experimentally measured. For example, as soon as you attempt to "examine" an electron, you knock it off its path. But that which can be neither measured nor computed simply doesn't exist. It had to be accepted that there is no such thing as an electron's path.

But if there is no path, how is one to describe the motion? What *can* be computed and measu-

red, however, is the probability of an electron occurring at a given point. If we take an electron revolving about an atomic nucleus, we can't draw its orbit, but we can shade in a ring within which there is a 99 : 1 or 999 : 1 chance of the electron's occurring (in the latter case the ring is wider).

The uncertainty of our knowledge of an electron's position determines the accuracy with which its velocity can be computed. The German physicist Werner Heisenberg demonstrated that the product of the uncertainties in a particle's position and velocity equals a certain constant number — Planck's constant — divided by the particle's mass. Hence, the more exactly the velocity is known the more vaguely is the position determined, and vice versa.

It may still appear that all this is no more than an inadequate description of the electron's motion: common sense tells us that it must have *some* path, simply physicists don't know how to compute or measure it.

Here is a description of an experiment that will show the erroneousness of such reasoning. Imagine a screen with two slots. A beam of electrons is directed on the screen and some of them pass through the slots and hit a photographic plate behind the screen. We take two pictures, one with one of the slots closed, the other with both slots open. Comparing the two pictures we observe on the second a pattern of dark and light stripes, as though the electrons had impinged on some portions of the plate and not on others. This is extremely strange for electrons behaving like beads. Matters are even worse

when we discover one detail: on the photograph of one slot we find a place undoubtedly struck by electrons, yet the same place on the photograph of two slots is untouched. This seems strange indeed, for one should expect that opening of the second slot would result in more electrons striking the plate.

The state of affairs as observed in the experiment categorically precludes the possibility of picturing electrons as particles travelling along a certain path. The phenomenon, however, is easily explained if we picture electrons as waves. In that case, if the crest of one wave overlaps the trough of another the waves cancel out, though taken individually each wave produces a certain effect.

The inevitable conclusion is that the concept of an electron as a particle possessing a path (and the same holds for other elementary particles) contradicts the experiment.

The question — in what way and in what experiments the electron displays its wave or its particle properties — is exhaustively answered by Schrödinger's equation.

There is a vast body of experimental facts of great complexity that explain the equation. It can be used to predict complex events in the life of elementary particles. There is not a physicist who questions the truth of this law of nature.

The laws of quantum mechanics elevate the wave-corpuscle dualism of particles to the status of an axiom. True, we should remember what has been said of the temporary nature of a scientific ceiling. It is quite possible that today's

axioms will prove corollaries of as yet undiscovered more general laws of nature.

One clever argument in support of the notion that the laws of wave mechanics represent a temporary ceiling was advanced, if I am not mistaken, by the eminent English physicist Paul Dirac.

The fundamental laws governing the behaviour of elements and particles include three basic constants: the velocity of light, the charge of the electron, and Planck's constant mentioned above. If we multiply Planck's constant by the velocity of light and divide the product by the square of the charge of the electron, we obtain the dimensionless number 137. (Dimensionless means that it is independent of the choice of a system of measurement.) Why is this ratio equal to 137 and not some other number? Future theory must answer this question. If it does, then instead of three basic constants only two will remain. The theory we are impatiently anticipating should automatically deduce one of the constants from the other two.

It seems logical to assume that it is Planck's constant that is the derived number (I do not go into the argumentation, which is in any case rather arbitrary). The thing is that it is Planck's constant which defines the limiting precision with which a particle's position and velocity can be simultaneously determined and which lies at the root of the uncertainty principle. The conclusion of this reasoning is that the future theory will dispense of Heisenberg's principle and thus alter our understanding of wave-corpuscle dualism.

A quarter of a century ago it seemed that this step in the progress of physical theory would soon be made. The development of science has not justified these expectations and the new interpretation of the motion of elementary particles still awaits its author.

However, whether or not wave-corpuscle dualism will ever be explained, whether or not it turns out to be a corollary of more general laws of nature — in any case any visualization of the motion of elementary particles is impossible.

This state of affairs appeared simply unbearable to mature physicists brought up on 19th-century conceptions. To form an idea of their so to say moral state it is interesting to leaf through some pages of the supplementary volume of the *Course of Physics* by Orest Khvolson. Written shortly before the author's death, the volume appeared in print in the beginning of the 1930s. Today only people interested in the old physics look through the five fat tomes of Khvolson's *Physics*. Published in the early twenties, they offered an exhaustive account of the theoretical and experimental physics produced by the 19th century. The material was presented in strict scholarly language. Physical phenomena were interpreted from a single standpoint and explained in terms of the mechanical motion of particles and the continuous nature of media. There were some vague points, of course, but they were regarded as passing, inconsequential and in any case trivial.

An excellent pedagogue and popularizer of physics, Khvolson could not remain indifferent

to the birth of new physical ideas. He understood that the edifice of physics had to be rebuilt. He could not rewrite his whole *Course*, so he wrote a special volume devoted to the turbulent events of the last decade. But how different this book is from the previous volumes! What has happened to the solemn, measured intonations and the firm conviction of the unshakable nature of the basic principles of physical science? I should like to quote a few passages to offer an idea of the extent to which contemporaries were shaken by the discovery of particle mechanics.

Khvolson writes that, in setting forth micromechanics, he encounters tremendous difficulties: "We could well replace the term 'difficulty' with the word 'impossibility'." The greatest difficulty lies "in the abstract nature of the basic concepts and quantities with which the new science operates". And further: "The fundaments, not only of science, but, in part, of scientific thinking in general, up to and including the fundamental law of causality, without which any scientific construction seemed unthinkable, and hitherto regarded as irrefutable, are being mercilessly destroyed. The right is being denied to base the explanation of observed phenomena on definite, clearly formulated hypotheses concerning their primary sources hidden from the eye and direct observation. The new idea that science deals exclusively with quantities that can be observed and measured is elevated to the status of a dogma."

But Khvolson sees this situation as temporary: "The time will come, the mist will disperse and

the truth will be revealed in all its depth and beauty."

In spite of the fact that the truth of the laws of wave mechanics was recognized at once and the field of their application began to expand with tremendous speed, physicists were still worried about being denied the possibility of visibly picturing the motion of electrons and other particles.

However, a new generation of physicists was emerging, physicists who did not have to learn anew. They accepted the axioms concerning the motion of microparticles without reservation. More, the "youngsters" couldn't understand why the "old fogies" were so worried that phenomena taking place on the level of infinitesimally small particles were different from events directly observable in the world of large things.

The conviction gradually gained strength that the fundamental role of physical theory was to connect different phenomena. Few people stopped to consider the possibility of seeking analogues for describing "things-in-themselves", that is events which cannot in principle be experimentally detected. People became more critical of meaningless assertions that did not allow of experimental verification. The conviction grew that the road to truth lay through the establishment of interconnections between phenomena.

This, of course, does not mean that analogue thinking was completely discounted. On the contrary, it became a tool for a number of graphic methods of describing phenomena. But the new models pursued the sole goal of facili-

tating analogue thinking. For example, it is today natural to make use of various three-dimensional constructions which reflect a particle's behaviour, not in our conventional space, but in an arbitrary space the axes of which are the particle's velocities of motion. But this is quite another matter.

There is no unbridgeable gap between the old and new mechanics. The one merges naturally into the other. As a particle grows heavier it becomes more and more clearly visible through the forest of mathematical formulas. After all, according to Heisenberg's principle, the greater a particle's mass the less the uncertainty.

The impossibility of simulating the motion of an electron does not mean that it is impossible to develop an analogue of the shapes, sizes and motions of atoms and molecules. We are fully entitled to describe the motions of molecules as we would the behaviour of visible bodies.

But then, the remarkable achievements of electron microscopy have already made it possible for us to see large molecules.

Chapter 9

THE PRESENT DAY

...in which the author describes his colleague's mentality — as he sees it. Incidentally, the reader learns of the author's attitude towards telepathy.

In discussing a person's abilities for scientific work someone may say with approval: "A real scientist with the precise mentality of a physicist"; or disparagingly: "Muddle-headed and incapable of physical thinking."

The absence of a "physical" mentality is a shortcoming as far as natural scientists are concerned. In other cases it is no greater importance than, say, the absence of a musical ear for the sporting achievements of a leading athlete. That is why one can also hear such testimonials voiced as: "A man of fine abilities and remarkable intuition, a talented technologist. Incapable of physical thinking, to be sure, dislikes it, in fact, but his excellent memory and natural intuition enable him to make truly remarkable finds."

Or, in another circle: "Oh, he's no physicist, he's a mathematician of the purest water. His works are like Brussels lace: fine and delicate. But physical thinking is absolutely alien to him."

What is this mentality of a good natural scientist which people define as "physical thinking"?

I hope that the preceding chapters have given the reader some idea of the historical changeability of scientific thinking. The "typical physicist", as I call the figure whom I regard as the embodiment of the standards of physical thinking, commences his work with a careful analysis of the facts in hand. First he asks himself: can a given phenomenon be observed repeatedly, can it be reproduced? This is a necessary prerequisite for a serious attitude to any undertaking.

A single fact may be a subject for polite small talk, not a point of departure for a scientific investigation. Very characteristic in this respect is the example of telepathy, which has lately come again into the public limelight. It is interesting that people far removed from science would very much like to believe in the possibility of transmitting thoughts at a distance. Time and again physicists are subjected to harangues loaded with temperamental "proofs" from their acquaintances, particularly female acquaintances. "She dreamed her son was ill, woke up and looked at the clock. A week later she received a letter saying that on that very day and hour her son had suffered a heart attack. After this how can you deny that thoughts can be transmitted at a distance?" The sceptical scientist listens to his excited pretty companion and smiles politely, barely concealing that he is more interested in her than in what she has to say. At last she flares up angrily: "You impossible man! Why aren't you interested in such things? Is

science so incapable of explaining such wonders?"

The physicist changes the subject: his answer would hardly have interested his companion.

When I am told of all sorts of miracles of this type I say something like this:

"If you wish a natural scientist to take up the problem seriously you must first of all convince him of the possibility of repeating an experiment. For example, let one hundred people lined up before a mind reader's eagle eyes think him their names. If he guesses even once, and I'm satisfied the performance wasn't faked, I shall immediately organize a hundred more experiments of the same kind. If it is proved that a medium can regularly guess other people's thoughts at least once in a hundred times, then natural science will tackle the problem."

To be sure, nowadays "progressive" parapsychologists take just such a course of action, fully realizing that otherwise it is impossible to gain a serious reader's attention. However, the numerous reports concerning achievements in the field are incapable of standing up to serious verification. This is only to be expected, as the transmission of thoughts at a distance contradicts the fundamental laws of nature, and telepathy possesses all the features of pseudoscience, which I shall speak of later on.

Thus, before a phenomenon deserves to be subjected to physical analysis it must be capable of being repeated or reproduced. Observation tells us that a certain event occurs in a given set of conditions. It may not, however, be inevitable, and further observation may reveal that

the event takes place with a certain probability. This is also sufficient to warrant a scientific approach. Incidentally, we must establish what is meant by "a certain probability".

For the notion to be formed that an event takes place with "a certain probability", and for that probability to be found and computed, an experiment must be repeated many times. Suppose you perform an experiment ten times and find that in three cases a characteristic event takes place. You cannot yet declare that the probability of the event is 3/10. You must continue your measurements. It is quite possible that in the next ten experiments the event will occur twice, and in the subsequent ten, five times. In some cases the event may not occur even once in ten tests. In order to make sure that an event has a certain probability of occurring a hundred, a thousand, ten thousand tests must be carried out. When it becomes apparent that with the number of tests increasing the proportion of success tends to a limit, then you can state with assurance that the event has a probability of occurrence equal to this limit.

As you see, it is not so easy to judge of probability, and our "typical physicist" never jumps at conclusions concerning the probability of an event on the basis of isolated observations for fear of making a mistake.

Look out of the window and see who passes by a window first, a man or a woman. What is the probability that it will be a man? Evidently, 1/2. How can this be checked? Presumably, you must count the number of men among, say, the first twenty passers-by. This would seem to be

quite enough. We look out of the window, and what do we see but 19 sportswomen shepherded by a single male instructor marching briskly to a training session. This is probability for you!

External interference may temporarily distort one's notions of a phenomenon. The "typical physicist" always remembers this and is never in a hurry to pronounce judgement on the probability of any event. Failure to remember this rule has time and again led to sad consequences, to false "discoveries" acclaimed in the press, highly praised, and then...

But finally a scientific fact has been established and now the scientist ponders how to describe it in the clearest terms. This step requires him to devise strict and exact quantitative concepts. Naturally, he seeks concepts that would be as simple as possible. No one has ever questioned the choice of the concept of velocity for describing the rate of motion. It was agreed that velocity should be measured by the number of metres travelled by a body in one second, although, of course, one could have designated velocity by the square or square root of that number of metres. To introduce a new physical quantity means to define the measuring procedure required to characterize an event in numerical terms.

Allow me to illustrate this rather cumbersome statement with a humorous example.

Imagine our "typical physicist" in a pedagogical laboratory. He is being shown researches carried out by one of the workers.

"We are engaged," the physicist is told, "in studying the patience displayed by pupils

of junior school forms. Our task is to determine the extent to which it depends on education, on conditions in the family, on heredity."

"Very interesting! How do you define a child's patience?" the physicist asks.

"Why," the educator says with some surprise, "we question him, his parents and his teachers to establish how he behaves in different conditions. Then we determine whether he is patient or not."

"I should take a different approach," the physicist declares.

"What would you do?"

"Hmm... What should we take for a unit of measurement? However, you can think of it yourself; I shall only offer an example. Say, you give a child a box of very bad matches and ask him to light one. It refuses to light. He strikes one match, another, a third... At last even an angel gets exasperated and flings the box away with a curse or what. The match on which this happens can be taken as a characteristic of patience. Something like that. Otherwise what is the use of a research in which there is no dimension by which to describe an event?"

The stunned educator says he will think it over. The physicist goes home convinced that the time has come for physical thinking to invade the realm of education.

After the quantitative units of measurement have been selected the experimental investigation can commence. Essentially, the work of any natural scientist is a quest for the dependencies and correlations of links between various concepts describing an event and the medium in which it takes place. How does electrical

conductivity depend on material, pressure, temperature? How is a body's heat capacity linked with its ability to scatter X-rays? How does the speed of a chemical reaction depend on solvent, temperature, lighting? How do sounds of different volume and tone affect the brain's electric potentials? How does the dip of a magnetic needle depend on the latitude and longitude of its position?... The whole of natural science can be set forth in the form of such questions and the answers to them.

I have described the characteristic traits of physical mentality as they are manifest in the experimental investigation of nature. Now let us turn to theory.

There are known facts that refuse to be classified, that are incomprehensible from the point of view of existing notions or, better still, contradict them.

The theoretician rubs his hands happily and gets down to work. Explaining these phenomena means elevating science to a new level. What can be more important? Known axioms and hypotheses are powerless — therefore new generalizations are required. They can be bold, they can even be mad. Their purpose is to shatter habitual notions, to startle men into action. Niels Bohr once remarked in connection with Heisenberg's attempt to explain the properties of elementary particles: "This is undoubtedly a mad theory. The only thing is whether it is mad enough to be true." This statement contains an important germ of truth: a new, radical breakthrough is impossible without the resolute breaking down of old concepts.

No contemporary physicist will oppose a new theory merely because it seems to contradict entrenched views; nor will he resort to arguments of a philosophical nature. The law of conservation of energy is a cornerstone of contemporary natural science, but if someone ventured to lift a hand against this fundamental of fundamentals, the theory would be subjected to an exacting scrutiny and not rejected simply on the strength of its being "impossible". The experience of the 20th century has taught natural scientists an important lesson: never attempt to force new ideas into old frameworks.

This does not mean, of course, that every new theory rejecting old values is deserving of attention. Previous achievements must remain untouched by a new hypothesis. They are too significant to be accidental. A new "mad" theory must turn into the ordinary, habitual theory when applied to events that agreed with the old axioms. This is also a lesson of the 20th century.

Both relativity theory and quantum mechanics were enunciated as generalizations. The old classical mechanics of Newton remains as a special case within them. The relativity theory turns into conventional mechanics at low velocities; quantum mechanics turns into it where particles of sufficient weight are concerned. Succession is an essential trait of a scientific theory.

Every serious researcher is first and foremost concerned that his new discovery should not disrupt the harmony of those fields in which science had fared excellently without his help. Every pseudo-scientist is primarily concerned with destroying the old.

Graphomaniacs are a scourge of literature. They inundate publishers with novels and poems, demanding attention, understanding, recognition. When they feel they are being slighted they shower the editors with complaints. Science has a similar scourge: authors of new-fangled theories of atomic structure, inventors of perpetual motion, discoverers of the universal ether possessing universal properties.

Even if the author of a new system is sufficiently educated and the language of his writings looks quite scientific, the pseudo-scientific nature of his theory is readily apparent. Characteristic of every representative of pseudo-science, whether he be madman, fool or rogue, is that he starts out with overthrowing the fundaments.

As a rule, the pseudo-scientist gaily swinging his club to clear the way for his revelations overlooks some simple logical error. His new theory rests on arguments borrowed from existing theories. An inventor of perpetual motion, for example, builds his complex machine according to the laws of mechanics. Yet he fails to realize that from those very laws it follows logically and irrefutably that his machine can never be built.

Thus, a new theory must include old conceptions as a special case and it must justify itself by explaining hitherto inexplicable phenomena. This, however, is not enough. Before an author's "mad" theories are accepted by the scientific community they must be used to predict some hitherto unobserved phenomena. The new theory wins final recognition when, and only when, the predictions come true. Then, and only then

are old conceptions reviewed and new ones substituted.

Obviously, the role of theory in natural science is not restricted to the discovery of new laws of nature. Theoretical natural science also has the task of so to say bringing the general laws of nature to the factory floor. In the great majority of cases we have no reason to doubt that a phenomenon takes place according to the laws of nature. But the laws governing it must be explained. In this case the question is of deducing a special rule from a more general law. To do this, it is necessary to simplify the phenomenon within reasonable limits (so as not to throw out the child with the water) and show by strict deductive mathematical reasoning what dependencies represent corollaries of known general laws.

The special features of physical thinking display themselves in both the formulation and the solving of the problem. Not every theoretical calculation is expedient. Theory may calculate the result of a concrete experiment. For example, one may undertake theoretically to compute the density of water. After several months of work one obtains the result: the density of water is, to an accuracy of five per cent, unity. But the density of water can be measured directly in a few moments, and with an accuracy of thousandths of one per cent. So what were the computations for?

On the other hand, it is quite obviously expedient to calculate the angle at which a missile must be fired to hit a prescribed target. The angle can, of course, be determined experimentally

by launching a thousand missiles at different angles, but it would be much too costly.

The same goes for theoretical calculations of various special relationships. For example, one could compute how the pressure of benzene vapour depends on temperature, but the findings of such a complex calculation cannot be used to predict the behaviour of vapours of other substances. Obviously, it is simpler to carry out a direct measurement.

In short, physics values theoretical computations if they yield sufficiently general laws embracing a wide circle of phenomena, when an exhaustive experimental description of these phenomena would take up immeasurably more time. Only then is the game worth the candle.

All this would appear to be self-evident. However, these rules are, regrettably, often violated by natural scientists — true, by those who lack the physical mentality.

So far we have been speaking of so to say the architectural style of scientific investigations. A few words should now be said about *how* the foundations of natural science are laid.

We can easily point out the completed buildings: first of all, mechanics with its unfailing ability to predict, with the greatest accuracy, the motions of bodies if the forces acting on them are known; electrodynamics, which can be used to compute electromagnetic fields if the electrical charges and currents responsible for them are given. One of the most beautiful departments of natural science, statistical physics, governs the behaviour and properties of gases, liquids and solids under the influence of external

conditions. The behaviour of atomic nuclei and electrons is successfully predicted by quantum mechanics.

All these branches of physics in a way resemble Euclidean geometry: a few axioms followed up by a strict deductive statement and the logical drawing of complex corollaries confirmed by experiments within the accuracy of the theoretical calculations.

In some cases the initial axioms are so simple that one has no difficulty in accepting them as ultimate truths. Thus, for instance, it can be shown that the three fundaments on which mechanics rests — the law of conservation of energy, the law of conservation of linear momentum and the law of conservation of angular momentum — can be reduced to the assertion that all places and directions in space are equivalent.

Not all basic axioms of science are, of course, simple. But then, must they be? Who can answer this? Paul Dirac declares that fundamental axioms need not be simple, but they must possess mathematical elegance and beauty.

An aesthetic criterion for mathematical formulas? Yes. The evaluation of equations and calculations as beautiful and elegant or, on the contrary, cumbersome and unwieldy is very popular among physicists.

Newton's law of universal gravitation is doubtlessly a beautiful law. You do not agree with me? You see nothing beautiful in the formula $F=\gamma\frac{m_1\cdot m_2}{r^2}$?

But consider how symmetrical and simple it is; the beauty of the law lies precisely in this

symmetry and simplicity. Imagine someone advancing a law of gravity with a denominator containing, instead of the square of the distance, the distance to the power of 9/2, and a numerator containing, not a product of the masses but, say, the square root of their sum. An unattractive, unpleasant formula. It would irritate us from the purely aesthetic point of view and cause us to question its validity.

The reader may think I'm joking. Indeed, why should nature have a preference for beautiful equations?

I don't know. Perhaps nature is a good mathematician. In any case, there can be no doubt that all the fundamental equations of modern physics stand up to the test of aesthetic criteria. You can take my word for it that the beauty and simplicity of mathematical representation of the laws of electrodynamics (Maxwell's equations) are a source of emotional excitement for the physicist — contrary to the accepted view that only works of art can arouse such feelings.

But if the axioms of the completed branches of physics are as beautiful as the author claims, does it mean that physics should be satisfied with them? No, quite the contrary. An illustration of this is Einstein's stubborn quest for the unified field theory. Our system of axioms would gain immeasurably if they could be represented as corollaries of a single law of nature. Einstein devoted the last decades of his life to the search for an equation from which the laws of mechanics and the laws of electrodynamics could be equally derived. Alas, his titanic labours were

in vain. The future will show whether such an overall axiom can ever be found.

To be sure, physicists are less concerned with the question of uniting mechanics and electricity than they are with the theory of elementary particles. After all, both mechanics and electrodynamics are examples of completeness, even though they remain under different roofs. The problems of today linked with these disciplines can be handed over to the applied scientists and computer mathematicians. The science of elementary particles, developing so rapidly before our very eyes and continuously yielding new remarkable discoveries, is quite another matter: still far from being a streamlined theory, it is no more than a collection of fragmentary conceptions. I think that we are justified in expecting that it is from this chaos of new experimental data concerning the behaviour of the smallest particles of matter that a new theory should arise, a theory which will perhaps not only bring mechanics and electrodynamics together but lead us as well to a unified law for all natural science.

That is why so many scientists are working today on theories in the domains still lacking them, notably in the sphere of motions of elementary particles at velocities approaching the speed of light. Schrödinger's mechanics doesn't work here because the particles are too fast, while Einstein's relativity theory doesn't because they are too light.

Achievements in this field are so far not great. But then, the task is no easy one. The future theory must be able to explain why experiments

yield just so many elementary particles; why a particle's mass is so much and not more or less; why a particle's charge equals that of an electron and differs only in sign, etc. In short, it is necessary to explain why the world of elementary particles is as it is and not otherwise. This *should* derive from common unified law of nature!

This law is being sought by researchers with a mathematical bent busy seeking elegant, beautiful equations. It is sought by physicists indifferent to aesthetic considerations and seeking success by sifting mountains of experimental facts obtained all over the world with the help of fantastically huge and fantastically expensive superpowerful accelerators through the sieve of theoretical analysis. The game is worth the candle, for the stakes are the discovery of a new great law of nature, a law which must result in a new revolution in our conception of the world.

Chapter 10

CHEMISTRY

...in which the author subverts his chemist friends and attempts to challenge an important part of their work. He claims that chemists should manufacture as many different kinds of good things as possible.

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I beg permission to start this chapter with a very ordinary example, a domestic chemical experiment conducted at a dinner table.

A white lump of sugar dropped into a glass of hot tea vanishes before your eyes. It dissociates into molecules, tiny representatives of matter. A molecule of sugar is made of three types of atoms: carbon, oxygen and hydrogen. They are arranged in a specific spatial pattern which does not easily lend itself to graphic representation. Nature can use the same atoms to build another structure, making molecules of a new substance differing from sugar in taste and other properties.

Not every arrangement of atoms is possible. This is because atoms possess a property called valence. Valence is the quality which determines the number of other atoms with which a given atom can unite chemically. An atom of carbon has a valence of four, oxygen has a valence of two, hydrogen, one.

Although the freedom of atoms to combine is restricted, nevertheless the possibilities for buil-

ding different molecules from the same set of atoms (such molecules are called isomers) are tremendous. Thus, for example, a molecule comprizing twenty atoms of carbon can be arranged in half a million different ways. In other words, there are that many different substances of the same atomic composition.

If we now recall that there are almost a hundred naturally occurring atoms, the infinite diversity of molecules will be apparent.

Substances made up of one variety of molecules are relatively rare in nature; mostly we deal with mixtures. One of the tasks of chemistry is to investigate the molecular composition of such mixtures and separate them into pure substances comprizing one type of molecule.

But the main task of chemistry is the production of new substances not found in nature or occurring rarely.

The possibility of manufacturing new substances is based on the fact that in some cases two colliding molecules find it better to break up and rearrange themselves. Imagine two men meeting; one is small but wears an outsized coat, the other is tall and his coat is splitting at the seams. Obviously, the best thing for them is to swap coats and part amicably. The advantage gained in the rearrangement of colliding molecules consists in that the atoms of the new molecular construction are more conveniently arranged.

By mixing different types of molecules and making them collide, chemists manufacture new molecules, which is to say they make new substances. There are many ways of doing so.

One can mix gases or liquids; one can dissolve preparations in a common solvent.

The manufacture of new molecules progressed rapidly in the 19th century, and by now chemists have produced some one million manmade compounds. To be sure, only a negligible proportion of them have practical applications. Nevertheless, the work was not in vain. In producing new substances the chemists at the same time discovered the rules governing their rearrangement and established the best and shortest ways of forming new molecules with planned architecture.

The ability of chemists to break up a molecule along specific bonds and bind the pieces together in a specific way is truly amazing to the nonspecialist. I have often had to ask chemists whether it was possible to join atoms in such and such a way, and after a moment's reflection I got the answer: Yes, it's simple, in two or three stages; or: Yes, but it's a difficult synthesis. Rarely was the way of producing a conceived construction unclear.

The chemist's task is further complicated by the fact that it is not enough to produce a new substance: he must also prove that it possesses the envisaged structure. I have gained the idea that a serious chemist spends more time on this question than on producing the substance. The structure is proved also by chemical means. The usual reasoning is: If the questioned structure is correct, then mixing with a certain compound should yield such and such a substance; if this doesn't happen—it is not the requisite substance. There must be a number of such checks. It is

only after a lengthy and exacting verification procedure that one can report in a scientific journal that the million compounds listed in the handbooks have been supplemented by a new, million first compound.

Most chemists investigate the laws of chemical reactions. Others belong to the ranks of applied scientists. As a rule they go to the laboratory to carry out a series of syntheses amongst which they hope to discover substances of practical interest. Applied chemistry also works on problems of purification of substances and elaborates the cheapest ways of synthesis.

Progress in chemistry is, to an even greater degree than in other sciences, unthinkable without systematic contributions from the applied chemists. The reason is that theory is still unable to state definitely what type of molecule is needed to produce a material of outstanding qualities. That is why practical chemistry comes up so frequently with new, striking finds.

The achievements of chemistry in the last decades are extremely impressive. One need but name synthetic rubber and manmade fibres. Or take the pharmaceutical industry: remarkable drugs that have revolutionized medicine are the result of the work of synthetists.

The trend towards substituting manmade materials for natural ones is increasing. Nowadays chemistry provides us with shoes and clothing; the time is not far off when it will start replacing natural beaf steaks with perhaps less tasty (at first!) foodstuffs from petroleum products.

Our country has set itself the important task of accelerating the development of chemistry

and chemical industry. In some cases this can be achieved simply by increasing the output of products manufactured according to known technological recipes. But this is not enough. Chemists must work ceaselessly to produce new and tougher manmade fibres, better fertilizers, pesticides that would be as deadly as possible with respect to pests and harmless to useful animals and man. To advance chemistry it is not enough to develop chemical technology; attention must also be given to synthetic chemistry. In the second half of the 20th century its success is unthinkable without the development of all natural science.

Up to the last two or three decades chemistry was a closed domain of natural science that possessed its own methods and empirical rules. But then physics forged ahead, gaining new positions, discovering the laws governing the arrangement of molecules and atoms and the laws of motion and interaction of atoms and electrons. It is only natural that these rules and laws began to penetrate into chemistry.

First of all, it became evident that there is no such thing as specifically chemical laws or specifically chemical matter. The general laws of motion and interaction of atoms and electrons determine the properties of matter and govern the disintegration and creation of molecules in chemical reactions. If this be the case, then the empirical laws amassed by the chemists must have common explanations and be special cases deriving from the general laws of nature. Hence, chemistry must be provided with a general physical foundation.

This work began some thirty years ago, it si now at its height, and is yielding appreciable results.

The theoretical problems of chemistry are extremely complex. Judge for yourself. A molecule colliding with another may strike it head-on, from the sides or the rear. The results of these collisions may differ. This is the first difficulty.

The reconstruction of a molecule may require several stages. Hence, not only the collisions between the initial molecules, but also between the intermediate fragments must be considered. This is the second difficulty.

The destruction of a molecule or its fragments may take place in different ways: it may break in such a way that an extra electron passes from one fragment to another, or perhaps this will not happen. This is another difficulty.

And even assuming that all this is known, even then it is practically impossible to compute the results of the collision mathematically on the basis of the laws of motion of electrons. In short, we are incapable of predicting the outcome of a chemical reaction on the basis of the general laws of nature, although we never for one moment doubt that everything takes place in strict accordance with them.

Well, how then are we to achieve even partial successes? This is done in two ways. The first involves a search for empirical laws linking the chemical behaviour of a molecule with its structure (a molecule's structure is determined by physical methods of investigation). An empirical law is established by experience. Suppose you

are interested in a class of substances comprizing 10,000 representatives, and you have chosen 100 of them. You establish an empirical rule for this hundred on the basis that it holds for them all: and although you cannot be absolutely sure, it is highly unlikely that the rule will not hold for the other 9,900 compounds.

This is usually the case, but if exceptions do occur they suggest the need for a deeper study of the rule.

Naturally, the search for empirical laws requires extensive, comprehensive investigations.

Another method is the creation of arbitrary models of molecules and using them to work out, if not the whole reaction, at least some of its stages; if not all the properties, at least one. I will cite only one example of the achievements of the physical approach to chemistry. Physicists have learned to measure the size of atoms, therefore a molecule can be represented in a three-dimensional model. One sees that some atoms are tucked away in the middle of the model and it is impossible to get to them. If for his reaction the chemist must link the deep-sitting atom with the atom of some other molecule, he knows in advance that this is hopeless. This is already a prediction and, though a negative one, it is nevertheless very valuable. It is but a partial success of the theory, as a chemical process depends not only on the positions of the atoms in a molecule, but on many other things as well, and a molecule's geometry is but one of many determining factors.

Another achievement is the possibility of predicting the colour of a substance. Chemists

synthesizing dyes make use of general laws associating colour with the presence of specific groups of atoms in a molecule.

Chemists who study theory usually call themselves physico-chemists. From my point of view they are just physicists studying chemical processes. For they use physical methods in their work, proceed in their constructions from the laws of physics, and reason exactly in the same way as physicists who have not the slightest interest in chemical transformations.

Of course, it's not a question of names, but to me all people contributing to the building of the foundations of natural science belong to the same cohort.

The absence of a perfect theory creates favourable conditions for all manners of "deviations" harmful to science.

There has appeared a category of skeptical pragmatists who declare that we can do fine without theory, that the thing is to produce new substances and find practical applications for them.

Such reasoning is bred primarily by self-opinionated ignorance. An attentive study of the latest achievements of chemistry reveals convincingly that quite a few new and extremely interesting materials could never have been created without progress in theoretical chemistry. It can be claimed with equal assurance that it would take much more time to synthesize various new substances if chemists did not apply theoretical conceptions in working out the best methods of synthesis.

In Kiev I was told the story of two institutes engaged in work on similar problems. One worked

well, the other poorly. The director of the good institute let more than half his workers engage in theoretical research. The influence of this half on their synthesist colleagues yielded excellent results. The institute's works were, as they say, on a high theoretical level. The other director was a skeptical pragmatist who would not let his workers do theoretical work. The absence of attention towards theory was detrimental to its practical achievements.

Another danger is the hatching of false theories by people who refuse to study the accomplishments of theoretical science or to accept that physics is the general foundation of natural science.

Such a theoretician is not a far cry from the inventors of perpetual motion mentioned in the previous chapter. He is usually a good synthesist or technologist who knows his job well, operates easily with chemical formulas and whose practical knowledge of reactions enables him to find examples to illustrate his theories. They may seem extremely academic, sport new terms, snappy symbols and sage and obscure statements. Such an author usually vehemently sets forth his ideas. He seeks to forestall possible objections by assuming a peremptory tone and making categorical statements. Fanatically gleaming eyes usually induce you to refrain from exposing the arbitrary nature of his constructions and flaws in his logic. Besides, what does a "genius" care for logic!

There is only one way of cornering such a "genius": by assuming that he may be right. You remind him that a sure sign of any theory

is the possibility of making predictions, and then ask him to state what reaction he can predict and what unknown phenomenon he can foretell. You will get no answer, for the author of the new theory uses his brainstorm reasoning to prove things long known.

In chemistry, as in other fields, the representatives of pseudo-science usually claim to be revolutionaries and they resort to political speculation to nip any possible objections in the bud. Unfortunately, one still encounters such people and is compelled to read their "works".

There is another category of academics in chemistry. They may be knowledgeable and indignantly denounce the pseudo-science that ignores generally accepted laws of nature, use scientific methods in their work, and yet produce work of no significance because they ignore one of the fundamental precepts of physical thinking. One of the commandments which theoretical chemists, unfortunately, frequently violate, is not to be carried away by useless calculations. Useless in the sense mentioned before: when a complex calculation occupying much time yields no new rules capable of predicting new facts and serves solely to interpret experimentally established details.

Psychologically one can understand why such calculations are popular. The synthesist always likes to know just why the reaction he has discovered goes this way and not some other way. And he is always most grateful to the theoretician who hauls a pound of paper filled with cumbersome formulas and seven-digit numbers into his laboratory and announces in a firm voice:

"The reaction goes this way and not that for a very simple reason: the easily detachable atom has weak bonds with the other part of the molecule."

The industrial chemist is delighted, he shakes the hand of the theoretician who has discovered the truth in sleepless nights of work and gets down to his job: for he must stage hundreds of experiments to establish how the reaction proceeds with other substances.

I once attended a boxing match. Hefty lads pounded each other, the gong rang, the referee lifted one man's hand and pronounced him winner. Then the next pair climbed into the ring. I tried to predict who would win, but I was more often wrong then right.

Despairing, I shifted my eyes to the audience. I suppose there were plenty of fans who knew the difference between a knock-out and a knock-down. But there were also chance spectators, like myself. One of them sat next to me, and after each fight he asked:

"Why did he win?"

Having come to the conclusion that prediction was hopeless, I replied in a confident voice:

"Why, it's obvious: the winner's arms are longer."

Concerning the next pair: "Don't you see? The winner's taller."

In the third pair the winner was a stocky athlete with short arms. My neighbour ventured timidly that he seemed to contradict my previous statements.

I interrupted him confidently:

"Don't you understand? Although he's smaller, he's the heavier of the two."

I felt elated and didn't doubt that I could provide valid explanations to the outcome of every fight ... when it was over, of course. Looking about the hall, I suddenly spied the spirit of a theoretical chemist I knew well hovering over the fans. There was an expression of satisfaction on his face.

Well, there's a black sheep in every flock. But it isn't they who count. The erection of a sound, good foundation in chemistry is continuing.

Chapter 11

BIOLOGY

...which claims that biologists can't get along without physicists. The author also expresses his regret that in his time he had failed to take up such an exceptionally interesting problem as heredity.

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Physics is steadily gaining a leading position in biology. This is not to be understood that physics is conquering biology and shoving biologists to the background. Simply the science we used to call biology is evolving into physics, thereby confirming the view that natural science is passing through a period of reconstruction on a unified foundation.

This reconstruction was, of course, gradual. There has been in existence for some time such an intermediate scientific domain as biological physics. It appeared at the boundary of physics and biology and employed physical instruments and physical methods of research to study biological phenomena on a purely biological level.

Matters began to change more radically in the last two decades, when it became possible to discuss biological phenomena on the molecular level and extend the laws of physics governing the behaviour of atoms and molecules to living matter.

The contacts between biologists and physicists began quite some time ago. An example is the All-Union Institute of Experimental Medicine organized in our country in the early nineteen-thirties. The institute had departments of biophysics, photobiology and biological physical chemistry with quite a few physicists on the staff, but they had an auxiliary role. The biologists would discuss possible applications of physical methods for their purposes with physicists, but that was all. Relations were cordial, but the biologists and physicists lived side by side like neighbours rather than kinsfolk.

After the war, biological physics began a new life in the walls of the Academy of Sciences, and the nature of the biologists' questions to the physicists changed.

Investigators began to tackle complex problems of structure. X-ray analysis revealed the arrangement of atoms in molecules taking part in biological processes. Protein molecules, the "bricks" of life, became targets of research. First the physicists learned to determine their approximate size, then shape, and finally their internal structure.

Electronic microscopy advanced in leaps and bounds. With each passing year the boundary of the invisible was moved farther and farther back: magnifications increased from thousands to tens of thousands to hundreds of thousands of diameters. Researchers managed to place individual cells and even parts of cells on the specimen holder. Viruses began to disgorge their mysteries, and the large molecules of which they are made became visible.

The biologists were, naturally, greatly interested in these developments. Not everything was clear to them, as the results were obtained by physicists, and the biologists were only just beginning to learn their language. Many biologist friends of mine would ask me to clarify one point or another, and I even had to make several reports on a topic which, after all, was outside of my field.

Several years ago Gleb Frank, director of the Institute of Biophysics, invited me to attend a lecture by the English researcher Max Perutz. I don't remember whether it was before or after he had received the Nobel Prize for his stupendous twenty-five years' work that resulted in his mapping of the structure of a protein molecule in all its atomic detail.

I had followed Perutz's work in magazines, but I was nevertheless interested in hearing his summing-up lecture. As I had expected, the lecture of that unassuming, stocky, dark-haired man, who gave a very business-like account of his work, greatly impressed the audience. His task had been to discover the sequence of the structural units in the protein, the twists and bends with which one unit follows another. The molecule he investigated comprises neither more nor less than 574 such units. With what could I compare this problem to offer an idea of the magnitude of his task? Imagine a tangled maze inside the famous Cheops pyramid. The maze consists of 574 bends, intricately twisted and twined. Your task is to give a detailed description of the maze without opening the pyramid. You may tap it over, sound it acoustically, X-ray

it — in short, explore it by indirect methods.

My interest in Perutz's work was all the greater as he employed methods that had been developed for such inanimate objects as rock salt, calcite and naphthalene.

The most important thing, though, was that the upshot of the work of Perutz and of other scientists, aimed at determining the structure of biological entities, was the discovery that the laws governing the structure of biological substances differ in no way from those governing the structure of inanimate matter. The same distances between chemically bonded atoms, the same laws of matching molecules. Molecules adjoin at the same distances, according to the same laws of packing as had been previously found for simple crystals not associated with biology.

All this served to reaffirm the continuity of the scientific front, accepted by many scholars. Researches in the apparently remote domain of crystallography suddenly turned out essential in investigations of protein structures. This work, in turn, was a link in the chain of problems unravelling the mysteries of processes going on in the living organism.

As material on molecular structure and arrangement in biological entities accumulated, confidence steadily mounted that it would be found that the laws governing processes would also prove to be the same for living and inanimate entities. It became apparent that all biological phenomena could eventually be described in terms of molecular displacements, fusions and disin-

tegrations in the manner developed by physics for substances unassociated with living matter. Structural studies left no place for a specifically biological substance and prompted workers to be bolder in building physical foundation beneath the edifice of biology.

It is not my purpose to list the various achievements of this important field of science. But for the claim that physics is the basis of all contemporary natural science to be accepted by the reader ungrudgingly, I should like to offer the following example.

One day a biologist friend of mine called and the following conversation ensued.

"Why don't you take up nucleic acids?" my visitor remarked.

"What are they?"

"Have you ever heard of DNA?"

"Vaguely."

"It has been suggested that the DNA molecule contains the secret of cellular activity."

"Is this molecule the nucleus of the cell?"

"No, it is part of the nucleus: I see your knowledge of biology is modest, to say the least."

"Perhaps," I said. "Please help me brush it up."

"Our bodies are made up of cells..." my friend began.

'Dispense with the ABCs," I interrupted him. "I happen to know that much, and our newspapers recently reminded me of Virchow's famous principle, *omnis cellula e cellula*, that is, that all cells necessarily derive from preexisting cells. I also know that there are different kinds of cells: muscle cells, brain cells, liver cells."

"Very well. However, to introduce the subject of our discussion we must, on the contrary, see what all these cells have in common."

"Namely?"

"All cells resemble a tiny capsule filled with liquid. The liquid is populated by different kinds of molecules and molecular associations forming remarkably interesting structures. We are still a long way from gaining an understanding of the functions of these molecules and corpuscles, but it is already obvious that the cell is a miniature factory that receives operational orders conveyed from different parts of the body by molecules or by the telegraph of the nervous system."

"But a factory needs power!"

"Certainly, and the cell receives the power it needs to carry out orders from solar energy, or food."

"Do you know how this takes place?" I inquired.

"The general lines of this process are known. It isn't this, however, that concerns us. If you're interested, I can bring some popular articles. What I want to say now is that thanks to the energy it receives, the cell is capable of obeying certain instructions and performing different kinds of work: mechanical, chemical, electrical."

"And mental?"

"Naturally. The infinite diversity of living processes of the body is effected by the aggregate actions of the myriads of cells that constitute it."

"I can't say that I've more than a smattering of it all."

The biologist laughed.

"We're still a long way from understanding the mechanisms of most living processes, but biology has made some remarkable gains in discovering certain general laws. Among them is the established fact that the cell's main work consists in the production of different types of protein molecules."

"I've read somewhere about the infinite diversity of protein molecules."

"That isn't quite so. Although the cell produces more varieties of molecules than a button factory does types of buttons, the assortment is, nevertheless, quite specific. The factory of the cell operates according to many different kinds of cut-and-dried blueprints. And in the last few years we've discovered the factory's 'chief designer'. It is a special molecule called DNA for short."

"And in full?"

"Deoxyribonucleic acid."

"I prefer DNA. So what was found?"

"I'll try to explain. The factory's 'management' is housed in the so-called nucleus. DNA is a very long molecule one important feature of which is that it is made up of four different types of bricks. I'm simplifying, but you can sort out the details yourself. I want to give you the gist of the discovery. These bricks are arranged according to a certain intricate pattern. Now, the first point. The cell's diverse 'operational plans,' transmitted without change from one cell to all its endless progeny appearing as a result of countless fissions, are coded in the specific sequence of these bits."

"That's all very well, but what does it have to do with the manufacture of infinitely diverse protein molecules?"

"Who told you that protein molecules were infinitely diversified?"

"I seem to have read it somewhere."

"In a sense this is so, but the remarkable thing is that all protein molecules are made up of twenty different units. Only twenty. By arranging them in different sequences it is possible to obtain the inexhaustible wealth of all kinds of molecules suitable for every life exigency and every taste."

"Please tell me about the manufacture of proteins," I said. "You've whetted my appetite."

"Do you know what a printer's matrix is?"

"Of course."

"Well, imagine that you've cast each line separately. If you try to fit one of the lines with the matrix so that each protruding part fits in with a corresponding indentation in the matrix, you will find that you can do so in one way only, by putting the line in its place."

"Obviously."

"A DNA molecule acts as just such a matrix, it uses a four-letter alphabet and has twenty lines. The part of the line in my schematic representation is played by a molecule called RNA for short."

"Never mind the full name. Go on, please."

"The DNA molecule is in a medium containing a sufficient quantity of building blocks of four different types. It begins by preparing the lines, that is, it manufactures RNA molecules."

"You speak of it as if it was really a typographical line. But it must be a complex chemical process."

"I'm glad you feel so. I'm omitting the chemical aspects of the process and dealing only with structure. Each line is a specialized RNA molecule capable of carrying one of the building bricks of protein molecules. When the RNA molecules are ready, they go hunting for their respective protein bricks and take them over to the DNA molecule (I'm simplifying the picture, but the principle holds), each line finds its place, and the protein bricks are thus laid in a specific, order; they combine to form a protein molecule."

"But many different types of protein molecules must be made."

"Quite right, and you can imagine a DNA molecule not as a single matrix sheet but as a sheaf of sheets, each one of which manufactures its specific protein."

"This is remarkable! Imagine a chemical process of such complexity taking place 'all by itself'! It's fabulous. I'm thinking that today, at the present level of technology, it would probably be impossible to design an automatic factory capable of manufacturing even a single type of protein. And here we have, in living matter and on a microscopic scale, an efficient enterprize obeying orders from outside. It sounds like a fairy-tale."

"It is your task to investigate these things and demonstrate that there is nothing supernatural in them, that the behaviour of all these molecules obeys exactly the same rules that operate for simple, non-biological systems."

"I'm afraid I'm too old to change my profession," I said.

"Hm. That, of course, is another matter. It's for you to decide. My only purpose was to draw a picture of the situation as it now is in biological science."

With this my biologist friend bid me farewell and departed, leaving me to chastigate myself for not having taken up these problems ten or fifteen years ago. For it has since then been shown that conceptions concerning the behaviour of organic molecules — ideas I knew and in some cases even helped to develop — can be applied to the complex processes going on in the living cell. The same rules, the same laws, the same foundation common to all physical science. In fact, in the early forties I myself had actually discovered the main feature of the whole process — the matching of molecules so that the wedges of one fit into the grooves of the other—as an essential property of all organic crystals. In complex systems, it turned out, the same rule holds not only qualitatively but also with the very same geometric correlations typical of the domain of simple organic crystals.

Today serious study is being given to the energy aspect of the matter. It has been shown that the miraculous processes take place in full accord with the law of conservation of energy. The energy needed to effect the transfer and building of molecules is supplied by the sun or food. Thus, the greater complexity in the collisions, dissociation and joining of molecules as compared with simple chemical reactions is of a purely quantitative nature.

Today molecular biology still has more mysteries than solved problems. Nevertheless, what we do know indicates beyond all doubt that the physical laws common to all the particles constituting the Universe lie at the basis of the most complex living processes.

That is all I wanted to point out.

Chapter 12

PSYCHOLOGY

...in which the reader will find, perhaps to his resentment, that the author sees no difference of substance between studying the motions of elementary particles and the aspirations of the human soul.

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It rather wounds one's ego to be called a little screw in a machine. Perhaps comparison with a larger part or a whole machine would not be so humiliating? Most people, I'm sure, would feel offended by a comparison with a chunk of hardware, however intricate. To be sure, it isn't much of a compliment. But... in the 20th century people go into raptures over the streamlined forms or clever performance of machines. In short, from my point of view compliments like "beautiful as a rocket" or "clever as a computer" make sense.

And yet, when, joking aside, you start telling a person that there is in principle no difference between man and machine, his most likely reaction will be one of protest. Oh, he'll agree that machines solve problems of any complexity better than man, that they are capable of carrying out the most puzzling orders. But a machine that can learn, a machine that can create, a machine that (and here the objections are most vociferous) can feel — well, this is going too far.

It seems impossible. How can one concede, how can one agree that a machine can be capable of emotions, capable of loving or hating, capable of admiring the beauties of nature? And yet, there is a large body of researchers (among whom I list myself) who are convinced that there is in principle no difference between man and machine.

Not that anyone claims that at the present stage in the development of science it is conceivable to build an artificial man or even a muscle a fraction of an inch long. Nor does it matter that the ways of modelling most living processes are still closed to us. It doesn't matter that the future may reveal to us that living phenomena involve processes much more complex and subtle than electric potentials and chemical reactions. What does matter is that the discoveries of the last few decades have led men to the conclusion that all living processes, the mechanisms of which are more or less known, obey the same laws of nature as artificially induced process and, hence, can be modelled.

There is no reason to doubt that the achievements scored at the beginning of the road will be multiplied in future. All the more so that natural scientists are convinced of the material basis of all physiological, emotional and rational manifestations of life. Today we are in a position boldly to uphold the claim that there is no difference in principle between machine and man. This is, as I see it, the greatest achievement of 20th-century natural science. All the revolutionary upheavals in physics mentioned before recede to the background before this staggering achieve-

ment of science. For in truth, can there be anything more important than discoveries enabling man to make a new assessment of himself? They are comparable only with the proof of man's evolutionary development from the simplest animals. Darwin, whether he meant to or not, demolished the belief in the divine origin of the human soul. Plain logic suggests that, if there does exist some spiritual essence, it must have evolved gradually, changing together with the physiology.

Yet for ages it seemed self-evident that it was impossible to reduce spiritual life to physical phenomena. To be sure, history knows of philosophers who consistently advocated the material nature of intelligence. However, they came long before their time, unprepared by the development of natural science, and therefore appeared vulgar and unjustified.

Our present confidence that man's spiritual life can, in principle, be reduced to physical processes in the brain and nervous system developed as an outgrowth of advances in two branches of science. On the one hand, intricate automatic machines were constructed the workings of which suggested analogues with the functioning of the brain; on the other hand, our knowledge of the mechanisms of the functioning of the brain and nervous system advanced tremendously: they turned out to resemble the automatic mechanisms built of vacuum tubes or transistors.

What is this likeness, which leads us to such far-reaching conclusions?

For a machine or the brain to function it must possess a knowledge of facts, laws and commands

capable of bodying forth in action. The whole wealth of human intelligence can be written down by means of two dozen letters of the alphabet, combinations of which form words, sequences of which form sentences. Even letters are not at all essential as symbols. The dot and dash of the Morse code perform exactly the same functions just as efficiently. Such a two-digital representation of numbers and letters in electronic digital machines is effected by electrical pulses which may "be" (dot!) or "not be" (dash!) in any of a machine's several tens of thousands of units. Thus, at every instant the data and the instructions being carried out by the machine are determined by the distribution of dots and dashes amongst its elements. Each element has only two possible states.

In the living organism, the activated elements are the nerve cells, or neurons. The wires connecting the neurons are called axons. It has been found that nerve pulses propagating along the axons obey the so-called all-or-none principle. All the pulses are of the same type and, like the pulses in a digital computer, they carry only two types of information: "dot" or "dash".

A thorough investigation of the problem presented here so briefly and crudely, reveals that, basically, the nervous system is constructed like an electronic digital machine.

Naturally, it took more than this general proof for the analogue to be accepted. Much has been done to reproduce the characteristic features of mental activity with the help of machines. Most impressive are machines capable

of learning, machines whose operation programmes include "memorizing" mistakes and instructions to avoid them in subsequent operations. It is a remarkable experience to see machines that play cards, dominoes or chess. Much has been written about them and I need not add to these accounts.

There are many games which give all contestants equal chances. For example the game of "sack, stone and scissors". Do you know it? An open hand is the sack, a clenched fist is the stone, two extended fingers are the scissors. Both players simultaneously show their hands. The winner is easily determined: the sack covers the stone, the stone blunts the scissors, the scissors cut the sack. Thus every figure has its match. It would seem that the chances of winning are absolutely equal, yet there are people who always gain the upper hand. How is this possible? The reason is simple enough. Most players adopt some simple tactic. For instance: I've just shown the scissors, my opponent will think that next time I'll show some other figure, so I'll fool him by showing the scissors again, etc.

An attentive person will quickly unfathom any tactic, and thus gain advantage in the game. What is the best line of action, though? Strangely enough, it is rejecting any tactic whatsoever. The best performance will be shown by presenting the figures in statistical disorder, at random.

The game is easily programmed, and the machine always proves to be the better player. If the game lasts long enough it is always sure to win. It guesses its opponent's tactics easily, but it obeys the laws of chance itself.

Machines can be, and have been, built that write poetry or compose music. Naturally, they must first be taught how. Even in our sophisticated age such a machine is capable of surprizing its poet or composer instructor. Once one enthusiast of machine poetry showed me a poem called *The Black Sun*, asking me not to mention his name so as not to invite his chief's wrath for misusing a machine built for solving complex equations; true, he switched it on for poetry writing only during brief night-time intervals. The machine was provided with the vocabulary of five or six poets, and the rules of selection of adjectives were drawn up in such a way that it drew its images from different poems of different poets. Some of its poetic gems were truly stunning. The enthusiast I'm speaking of regarded the machine as an excellent supplier of semi-finished products. The machine fed out beautiful, albeit practically meaningless, phrases concocted from the vocabulary of famous poets, which the man by dint of some cursory editing quickly converted into symbolist style poetry.

To be sure, this is on the level of a prank, but a prank of potential promise. There can be no doubt that machines can write poetry, and there are no restrictions of principle on the degree of intelligence or vividness its verses can achieve. The number of impressive examples of the replacement of human brains by machines could be multiplied. However, it would be foolish to simplify the problem, equate machine with man and expect an artificial human brain to be created in the near future. The distance between brain and machine is tremendous. Hundreds of

millions of years of evolution produced the remarkable machine, our brain, in which the number of "vacuum tubes"— neurons — is expressed by a one with ten zeroes appended to it, all compressed into a volume of about 1,200 cubic centimetres. Each "tube" consumes no more than one thousand-millionth of one watt of electric energy. Compare this with our hardware made up of tens of thousands of tubes: it is a thousand million times larger than the brain and consumes as many times more power.

An important difference between the brain and a machine is the brain's exceptional aptitude for simultaneous operations.

The reliability of the brain, with its tremendous capacity for interchangeability of parts, is beyond any comparison with the best of modern machines.

Last, but not least, the brain has a memory capacity a million times greater than that of the best modern machines, though the experts do not consider this such a great difference.

On the other hand, already now machines surpass the brain in at least one respect: they work ten thousand times faster.

"And still I think that it is more important to determine and study similarities than to mind differences. It is certainly not a question of creating an artificial man. I, for one, don't see what purpose this would serve. The practical aim is to create automatic devices to help man's mental work. The important thing is the principle: confidence that, within the framework of a materialist outlook, there exist no arguments of principle against the possibility of creating artificial living creatures capable of propagation

and progressive evolution, possessing emotions will and intelligence, up to and including its most refined manifestations." These words were written by Academician Kolmogorov, and I quote them because in such categorical views (with which many people disagree) it is good to enjoy the support of an authority in the subject.

I fully realize that a few pages in a book, even if they are concluded with a reference to a leading light in the field, are incapable of convincing everyone. A problem of such magnitude must be pondered and all the latest works on it must be thoroughly studied.

Before concluding this chapter, I should like to say a few more words about "machine emotions". At first glance the very juxtaposition of the words seems utterly fantastic. Can the author mean that a machine is capable of being moved by the sight of a beautiful sunset? On the other hand, many will agree that a machine can see a sunset, just as a television set does. Furthermore, you will agree, I presume, that a machine's memory can be supplied with thousands of landscape projections with which it can compare the sight before it. Nor can one challenge the possibility of programming aesthetic criteria, such as colour shades, perfection of form, etc. Hence, a machine is capable of evaluating what it sees. The external manifestations of such an evaluation (including little cries of "Oh, how beautiful!", a deep sigh, tears and any other expressions of emotion) can be duplicated by technical means. What then remains? "What do you mean 'what remains'?" the irate

reader may exclaim. "What about inner emotions?" But we don't know anything about them, and probably never will... Do you know the feelings of your companion admiring the sunset next to you? That is the crux of the matter: the definition of life and intelligence can only be functional. A perfect automaton would, like your companion, insist that you see things in the same way and that there is no difference in your delight at the view.

Having accepted the basic idea, we must concede that the spiritual life of an intelligent being can and should be a subject of natural science. The problem is an analytical one. The investigator tackling it finds himself in approximately the same straits as an engineer studying the possibilities and rules of operation of a strange, intricate machine.

Great successes have been achieved in localizing the sections of the brain responsible for various emotions and actions. There are good reasons to expect that fabulous progress will be attained along this road in the coming decades. Nature is the greatest designer and builder. In hundreds of millions of years it has created a machine of unsurpassed complexity and perfection. Future analyses will enable us to isolate the general human properties, the parts of the mechanism equally characteristic of all people. We already know that every newly appearing "machine" possesses countless inborn, inherited features. It is, furthermore, geared to memorize facts and rules learned from life. Its education consists in continuously supplementing its programmes of action, intelligence and emotional experience.

We are, of course, still infinitely far away from being able to provide a description of the physical processes of mental activity. But the very idea of the physical nature of emotional life has, doubtlessly, profoundly influenced the development of psychology.

After all, even in simpler domains of natural science, even in chemistry, we are still incapable of providing exhaustive descriptions of processes. This does not mean that natural science retreats in the face of difficulties. If it is impossible to offer a thorough theoretical description of a process, scientists search for empirical and semi-empirical laws. And although they are special cases whose derivation from the general laws cannot as yet be proved, they nevertheless serve a useful purpose in science by providing quantitative descriptions of processes and making it possible to predict events.

Such an approach is possible in modern psychology, which means that physical methods of reasoning have a profound influence on problems connected with the study of human nature and behaviour.

In the last few decades psychologists have sought to employ definitive concepts, introducing numerical, albeit arbitrary, criteria of evaluation. Measurements lead to experiments, and sure enough, in most psychological investigations we find extremely interesting experiments.

There are two experimental problems that could be posed with the purpose of establishing rules of behaviour or laws of human nature. Firstly, one can study the behaviour of a single person, say his reactions to similar situations involving

one variable parameter. For example, his mental acuity depending on the time of the day or, in climbing uphill, the altitude. After a sufficiently long study of one specimen it would probably be possible to produce a fairly detailed psychological chart of him. But establishing the laws of behaviour and reaction for an individual Tom or Dick is not, in itself, of much interest or value. It is the same as making an exhaustive study of the molecular and electron mechanisms of a single chemical reaction. The result of such a study provides no material for formulating a theory that would hold for all chemistry.

More of interest would probably be an investigation of the behaviour and characters of thousands of people, the establishment of general laws for representatives of different age and social groups. This is the substance of the second problem.

Such investigations, interesting as they are by themselves, are essential to prepare the ground for future conquests of psychology. Empirical approaches in other fields of natural science have served and will continue to serve as a basis for a physical understanding of respective phenomena in psychology.

Here are several examples to offer an idea of the methods of modern psychology. They may appear rather theatrical but are of definite interest. However, judge for yourself.

It is generally accepted that material considerations are a powerful incentive in overcoming difficulties. How can this be verified?

The investigators select a series of tasks which require a subject to concentrate all his attention

to carry them out and which are greatly facilitated by habit. For instance, a disk with a metal plate one square centimetre in size soldered near the rim is revolving at a speed of one revolution per second. The test subject takes a metal pointer with which he must press the plate to stop the disk and hold it that way as long as possible. The pointer is an electrode which closes an electric circuit enabling the time during which the pointer is in contact with the plate to be measured exactly. As a rule, the first attempts are rarely successful; then most subjects quickly learn the trick and manage to halt the disk for any period of time.

The test was made on groups of persons variously interested in the outcome of the test. The members of one group were completely indifferent to its success: they were told that this was a simple psychological study in which they would figure as nameless statistical units. The members of another group were promised cash rewards for successfully carrying out the task. Finally, the subjects of the third group were made to think they had a vital interest in the success of the test. This was done in the following manner.

Ten applications were received to fill a certain vacancy. The authors of the experiment got permission to introduce their test along with the examinations of the candidates' business qualifications. The applicants were, of course, not aware that this was a psychological test.

Arbitrary quantitative evaluations of the degree of interest were introduced and graphs were plotted that showed the degree of success in performing the task depending on the incentive.

Two results were obtained from this investigation. First: success was best when the degree of interest was median. Second: in the case of the more difficult task, the less the incentive the greater the success. This is surely a surprising result. Evidently, excitement dominates over self-interest. Examiners of students would do well to mind this.

Fate frequently confronts us with the need to make a choice. One has to decide which of two dresses to buy; which of two roads to take for a walk; whether to go to the cinema or a stadium. Before the choice is made either alternative appears of equal value. Then the choice is made and the hesitation is thrown aside. The question is: does one regret the discarded alternative? Mass tests were staged to determine this. One of them consisted in the following.

Girls were offered to pronounce judgement on twelve objects; in one case this was twelve dolls. Then two of the dolls of average attraction were chosen and the girls were allowed to take one. After that the test was repeated, all twelve dolls were set in a row and the girls were again asked to evaluate them. What happened? Why, the rejected doll was now rated lower than originally and the chosen one was rated higher: it seemed better. The obvious conclusion is that, far from regretting a rejected alternative, as soon as we reject it its value decreases in our eyes. This, when one thinks of it, is a very reasonable and useful instinct.

The result of this experiment can be generalized. A person wavers between two decisions. Both have their pros and cons. Then the choice

is made. Now the pros of the rejected alternative as it were come into dissonance with the new situation. A dissonance is unpleasant, and a powerful instinct goes to work with the purpose of getting rid of the unpleasant emotion. How can the dissonance be resolved? Why, by telling oneself that the choice was justified and the rejected alternative is in fact worse than it had initially seemed.

This general rule has been confirmed by many different psychological experiments.

I have cited these examples as proof that the methods of psychology are coming closer to those of exact natural science. They have the same typical features: strict definition of concepts; elaboration of a method of quantitative characterization of a concept; experiment; statistical processing of experimental observations; discovery of special laws and their generalization into certain rules.

Of course, many questions remain unanswered. It must be emphasized, however, that nowadays psychology is developing along the same lines as other branches of natural science. Investigators are preparing the ground for physical interpretations of psychological phenomena by staging objective experiments.

Chapter 13

PHYSICISTS ENGAGE IN SCIENCE

...which demonstrates that, as it is impossible to embrace the infinite, physicists are compelled to separate into detachments of theoreticians, experimenters and hardware men.

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All graduates of the physical departments of universities call themselves physicists. In fact, it is so stated in their diplomas. To be sure, at the time of graduation they all have much in common: they have attended the same courses, carried out the same laboratory work, and in general received the same education.

Several months after graduation they are at work. Where? At the university offices you will be told that alumni can be found at a chemical engineering works, a weather station, a metallurgical institute, an institute of chemical reagents, an aircraft factory, an institute of criminology, an archaeological expedition, an atomic ship.

Visit them at their laboratories, and you will find that they work with practically the same kinds of apparatus. Establishments operating in different fields provide their physicists with the same spectrographs, X-ray apparatus, cryogenic installations, computers... The impression

is that we are dealing with people of apparently the same profession.

But get the graduates together ten years later. They will certainly be delighted and will hilariously recall the pranks of their student days, praise the good and ridicule the bad teachers; but they will hardly talk shop: they have diverged much too widely apart. In many cases they work in entirely different fields, pursue different goals, and differ in the nature of their specialized knowledge.

If you go through the laboratories of all the establishments where graduate physicists work you will also find that they have acquired colleagues of widely different professions: chemists, biologists, physicians, engineers. The current knowledge of our erstwhile physicists follows the lines of their non-physicist colleagues, and they tackle the same problems.

The same physical ideas and methods of research apply in all branches of natural science and technology without exception, and this helps the young physicist to find his place on practically any job. The chapters of physics which are superfluous to his work are soon forgotten and replaced by knowledge in the narrow speciality of his choice. On the other hand, the young specialist who engages in physics in his narrow field supplements his body of learning with essential knowledge of physics. Thus the two specialists draw even.

Young people graduating from physical departments usually follow one of two main roads: some devote themselves to natural science; the greater number go in for the applications of

physics, which is to say that they join the ranks of applied science.

In the former case the young man's education as a physicist directly serves his professional objectives. Nowadays a good understanding of the general laws of nature and a working knowledge of the mathematical apparatus of theoretical physics are essential for every natural scientist, whether he intends to investigate nuclear, chemical, biological or geological processes. He will find no difficulty in making good his lack of specialized information.

In the latter case, knowledge of physics arms the worker with a knowledge of research methodology. As here the role of special disciplines is much higher, it may be that physicists for the applied sciences would be better prepared by specialized colleges.

However, even the physicists working on the fundaments of natural science, that is to say the "pure physicists", are not all the same, and though they are united by a common profession, stratification amongst them is inevitable.

There are two fairly clear approaches to the investigation of the laws of nature. The first is experimental; it is based on laboratory experiments — the questions man asks nature. Nature is jealous of its secrets. To fathom them and obtain the answers to questions one often has to create special, artificial conditions. Very high pressures and temperatures, powerful beams of light and radio waves: only when subjected to such onslaughts does nature surrender and satisfy the researchers' curiosity.

The natural sciences also require another — theoretical — approach. A careful consideration of experimental facts enables the researcher to conceive the course a phenomenon takes and develop a model of it. If the accepted hypotheses are correct, logical reasoning and suitable mathematical calculations enable him to draw conclusions which may then be compared with experience. If the conclusions are confirmed experimentally, the hypotheses are at least plausible. If not, they must be rejected.

These two approaches develop in constant interaction. Thus, new experimental facts which fail to fit existing theoretical conceptions call for a revision of accepted schemes and models. Reciprocally, new theories lead to corollaries not yet verified by experience and confront science with new experimental tasks.

Undoubtedly, the ideal scientist is the one combining both approaches, but for a number of reasons to be shortly discussed 20th-century physicists have separated more or less clearly into experimenters and theoreticians.

Today, the investigator studying nature by experimental methods is quite unlike the experimenter of old. Even 50 years ago it was quite natural for a scientist of whatever status to carry out an experiment from beginning to end with his own hands. When I was a university student we had in the physical department an excellent experimental physicist, Konstantin Yakovlev. At exactly 11 a.m. he appeared in the corridor of the physics building wearing an immaculately pressed suit and gleaming white shirt with a standing collar propping up his cheeks. In his room

he removed his jacket, put on a coverall and went to the lathe standing there. He did all the necessary turning, carpenter and glass blowing work himself. In person. From beginning to end.

Obviously, there is a certain charm in this. But the present rate of scientific progress has reduced the number of such researchers to naught: division of labour in science has become as essential as in industry.

Still, researchers who like to create measuring and other experimental apparatus are to be found in our day, and they constitute a very useful scientific body. And it is probably only natural that some of them are wont to carry things to extremes. When I was working at the All-Union Institute of Experimental Medicine, next to my laboratory was that of Yevgeny Komarov, one of the first men in our country to set up a complex installation for measuring Raman spectra. Every six months or so he called me to his laboratory to show his achievements.

I never failed to go into raptures over the installation: the carefully fitted parts, the rubber water tubes tidily bunched together and held with attractive clasps, glass glittering against a background of black varnished wood. Everything was made purposefully and attractively. I derived aesthetic satisfaction from the sight of his installation.

"Here is the spectrogram," Komarov said. "Up to standard, on a par with the best world examples."

Komarov himself assembled the instrument, he had applied all the finishing touches, though the parts were manufactured at the workshop.

As compared with the "total" experimenter, this was already a substantial step forward, since Komarov calculated, designed and drew the blueprints of his device.

"Get on with the investigation," I remarked to him. "This is a new method, people are only just starting to use it, and you have a chance of forging ahead, especially as there are so many vague questions."

Komarov agreed and, eyeing his device appraisingly, absently bid me farewell.

A couple of weeks later I saw several crates standing at the door of Komarov's laboratory. I dropped in and saw that everything had been dismantled.

"What's the matter, Yevgeny Vladimirovich, what happened?"

"Oh, I'm sending it back to the workshop."

"The workshop? Is something wrong?"

"No, nothing's wrong, but it occurred to me..." he took hold of the lapel of my jacket and began telling me excitedly of some new improvement he'd thought of. "It'll make all the difference in the world," he concluded.

The launching of the apparatus was delayed for another six months. Komarov got down to a new set of calculations. This happened several times. The upshot was that he never got his instrument working: in the summer of 1941 he was called up to the army and never returned from the war.

If I were asked to classify physicists, I should call this type of researchers, men devoted to measurement per se (they don't care *what* they measure), hardware men. It goes without saying

that they form an essential detachment in the army of science; without their stubborn work many remarkable achievements in experimental natural science would never have been gained.

But the typical 20th-century experimenter is a different kind of person. He only buys, orders and, in exceptional cases, assembles his apparatus. He may even have but a remote idea of the way the device he is employing works. If something goes wrong he seeks someone else's advice and assistance.

The talent of this kind of researcher lies primarily in his ability to formulate a problem in precise and definite terms. The novelty of such an investigation consists in the creation of new unusual conditions for a phenomenon which would reveal some unexpected aspect of it. Or, say, in the simultaneous comparison of different properties and qualities of a substance.

Sometimes the result of an experiment is immediately apparent, as when one obtains an electron micrograph or measures some specific properties of matter. In many cases, however, much difficult but exciting deciphering is required. Often a tremendous volume of calculations must be carried out with the help of high-speed electronic computers. Then the experimenter spends most of his time at his desk and may even earn the reputation of a theoretician — though only among the hardware men.

Most experimenters are satisfied to have discovered some new and interesting facts and call it a day. Sometimes, if they get the opportunity, they compare their findings with existing theories and even follow theoretical works of other scien-

tists, but their purpose usually is to seek ideas for staging new experiments.

Obviously it is more interesting to head a research laboratory that processes the results of its experiments for itself. Then the experiment serves to verify your ideas and provides a stimulus for developing and perfecting your theory. It is not often, however, that experimental material obtained in one laboratory is adequate for enunciating and verifying a theory. The difference between "your own" and "somebody else's" experiment has begun to disappear.

This is perhaps why researchers with a bent for theoretical reasoning frequently never even feel like staging experiments themselves. To them belong the experimental works of the whole world. If a special experiment has to be staged to verify some postulate, it is not so difficult to come to terms with a "pure" experimenter working in the same field of physics and have him help out the theory.

Nine-tenths of the theoretical physicist's work is brainwork; the remaining tenth is devoted to calculations, arguing the problem, and writing the paper. The state of intense mental work is only too familiar to the members of a theoretician's family, down to the smallest children.

"Masha, let's go to Daddy."

"Oh, but he is working."

"No, he isn't: he's just sitting in the armchair."

"But he is: look at his eyes."

Film makers only too often exploit a vacant, intense stare as a sign of a working brain.

A theoretician's working day is probably longer than that of any other worker. The best results

are, of course, achieved sitting at his desk; but the mental process continues all the time: at lunch, at meetings, on the way to and from work; even when he is apparently engaged in something else his mind continues to sift through conclusions, arguments, objections, etc.

How is a new scientific idea born? What is the mechanism of creativity? I have no doubt that sooner or later an answer to these interesting and as yet mysterious questions will be obtained.

A sequence of logical steps leads to a goal. The road, however, is an intricate maze through which the brain has to make its way. Every step made breeds tens and hundreds of possibilities, each of which must be considered before the next step can be made. Like a mountain-climber who seeks the surest place to set his foot, so the mind must weigh the facts that precede each step, the ideas that must be rejected or added to the new theory.

For the road to be traversed successfully the mind must be kept rigidly in control. The slightest distraction can lead to a slip in the logical reasoning; a slip of the memory can mean the overlooking of an important fact.

Who can hope to traverse the road successfully? Obviously only he who has a good memory, who is capable of strict reasoning, who possesses the ability never to lose the thread of a complex logical scheme. To these qualities of the mind must be added one trait of the character.

I have often heard the complaint: "You know, I was once in the company of some young physicists. Nice people all of them, but you know what I disliked? They speak of everything with

such assurance, with such aplomb. They seem so self-confident."

I think this isn't accidental. Self-confidence is essential for a good researcher. Of course, I mean confidence in one's scientific reasoning, not the self-confidence of the upstart who cares for nothing but his own desires.

For the ratiocinating physicist there should be nothing stronger than his own logic. Suppose he has carried out some reasoning and mentally checked the path that has led him to the result several times. The result, however, contradicts accepted views and clashes with the opinions of leading lights. "I must have made a mistake somewhere," the researcher concludes. "I must try to tackle the problem from another side."

Such a person will never make an outstanding scientist. He is destined for secondary roles. A genuine scientist will not retreat in the face of unproved authoritative statements.

He checks his reasoning ten or a hundred times, always proceeding from the principle: either my logic is flawless or I've made a mistake. He rejects objections based only on references to authorities. Anyone who wishes to challenge him must point out the error in his reasoning. Until this is done he trusts his reasoning — and all the worse for the facts if they fail to fit into his scheme.

It is well known that most theoretical physicists (and mathematicians) carry out their best works early in their careers. Here are some convincing examples.

Newton made his greatest discovery at the age of 27; Maxwell, at 29; Heisenberg, at 24; Einstein, at 25; Lobachevsky, at 33; Galois, at 19.

Exceptions are rare. Schrödinger proposed his equation when he was 38.

An explanation of this interesting state of affairs can be found, I think, by analogy with sport. With time a person loses his ability for instantaneously rallying all his forces. In our youth the rallying of all one's physical forces in a matter of seconds can lead to phenomenal records.

Perhaps with age one loses the ability to rally one's spiritual forces to the utmost. The thing we call a stroke of genius is like a bolt of lightning — it requires an intensity of the highest order, of which only youth is capable.

The mature researcher possesses greater knowledge, greater experience, perhaps even greater talent, but he has lost the ability of rallying all these qualities for brief moments and turning them on to the fullest capacity. Speaking in the language of physics, the mature man may possess greater mental energy, but the younger brain is capable of greater power output. That is why in mathematics and theoretical physics young people have an advantage over their seniors. But where success depends primarily on a profound and comprehensive analysis of facts — and this is the case in experimental natural science, where intense synthetic reasoning plays a secondary part — in this case, as could be expected, the advantage is with maturity. Darwin, Mendeleev, Pavlov, Roentgen were all mature scientists at the time of their greatest discoveries.

A conclusion of social importance can be drawn from what has been said: a nation's wealth is

measured not only by its industrial potential, the mileage of its roads and the magnitude of its natural resources; human talent is worth more than material wealth. It is not accidental that the Americans did everything to ship the best scientists of conquered Germany to the United States even before the bullets had stopped flying.

A potential theoretician's talents may lay latent all through his boyhood and adolescence. A loss of three or five years may in this case be fatal. For this reason it is most important to hunt for young talents and create the best opportunities for their development.

Many of our leading theoreticians realize this only too well. Our papers have written a lot about efforts being made to discover young people with theoretical abilities in natural science in every village and small town. As far as I know, nothing of the kind is done in capitalist states.

Concern for the common weal has become a part of the nature of Soviet man. Leading scientists devote much time and energy to organizing contests which help to locate young talents; special schools are set up in which gifted children study according to special syllabuses.

Serious mathematicians and physicists in love with their professions and realizing their public duty derive satisfaction from supporting these public measures. They readily do all they personally can to help young talents find their road. Here is what Academician Lev Landau, one of our leading theoretical physicists, used to do. A young man could call at his home and state

his desire to become his pupil — that was the only preliminary. The next thing the young enthusiast had to do was to pass an examination. After that Landau told him what to read, what problems to learn to solve, and entered the future scientist's name in a special ledger. If on the next visit the young man gave a satisfactory report of his work, a first cross appeared opposite his name.

New instructions and more difficult tasks followed. I think it required five or six little crosses opposite his name for the candidate to pass the test. After that he received research tasks and was allowed to attend seminars. Thus he was set on the road. The rest depended on his talent and industriousness.

Just as the hardware men are the experimenters' main assistants, so the main helpers of the theoreticians are the researchers engaged in mathematical physics. And just as there are workers who don't care what they measure, let them only tinker with beautiful instruments, so there is a class of people who don't care what they calculate as long as the calculations are interesting and exact. And just as the proportion of time experimenters devote to the hardware varies from zero to 100 per cent, so the proportion of time a theoretician devotes to calculations may vary within very wide ranges.

Many theoreticians like to carry out their calculations from beginning to end in their desire to obtain the final "number": that is the quantity capable of direct experimental verification. Others don't care for such work and are satisfied if they can derive the general mathematical formulas for a physical problem.

Obviously, every kind of scientist is needed. The ranks of natural science workers range from the hardware zealots on one end to the mathematicians on the other.

One of the most difficult problems is that of cooperation between these investigators, whose tastes differ, and who are often totally uninteresting to one another. Experimenters may find it quite impossible to grasp the essence of a theory concealed from them behind a rambling forest of formulas. Their interest in theoretical findings is restricted to data that can be compared with experience. On the other hand, the intricate methodologies of contemporary experiments make it impossible for a theoretician to judge their authenticity, he takes for granted the figures supplied by his experimenter colleague, and often builds his theoretical reasoning on them.

There are several bonds capable of linking the members of these groups of researchers. The first bond is a common methodology. Say, I engage in X-ray analysis, which is also employed in a metallurgical institute. We use the same X-ray tubes, apparatus, cameras. There is much in common in the methods of deciphering X-ray pictures. But I am quite uninterested in the structures and properties of metals. The formulas of the organic compounds that interest me are Greek to the metallurgist. The linking factor is the methodology. However, as it is used for entirely different purposes the bond grows weaker.

The second bond is when a common subject is investigated by different methodologies. For example, scientists are studying the structure of organic molecules, but one group does it by

optical methods, the other by X-ray methods. Here, the bond is weakened by the difference in the tools of research.

The problem of bonds doesn't end here. Theoretical interests may lead a researcher into an entirely different camp. One often encounters theories with entirely different applications but very alike in their mathematical methods of representing and calculating phenomena. Such theories may become communication lines if representatives of different groups of researchers get interested in them.

In short, the contacts between a modern natural scientist and other scientists may be represented as a complex polyhedron on whose sides adjoin figures of entirely different configurations. Thus, the researcher whose profession is X-ray analysis of organic substances finds colleagues among metallurgists and opticians studying organic compounds, two groups of people most unlikely ever to come into contact.

As you see, we again and again return to the problem of the continuity and complex interactions of all sections of the scientific front. Advances in the study of metals may influence achievements in molecular biology. A lag in spectroscopy may hold up the development of roentgenography.

An essential aspect in the development of any branch of science is the general background of scientific advance.

Chapter 14

BROAD IS YOUR ROAD

...which states that physicists are favoured not only by science fans but also by factory directors worried about the fulfilment of their production plans. The reader also learns that the pursuit of physics is no obstacle to participation in expeditions.

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I shall not tell the reader of physicists heading the elaboration of applied problems of primary national importance, who have shouldered enormous responsibilities. To carry out their work with distinction, this group of scientists must possess, in addition to all the qualities of a foremost physicist, outstanding organizational abilities. Some day a book will be written about researchers of this type, people like Kurchatov and Vavilov, a book that will tell of the part played by physicists in building up our country's military strength. This is an interesting and important topic, but it lies beyond the scope of the present book. But it would be unjust not to devote a single chapter to the nine-tenths, if not ninety-nine hundredths, of physicists working in industrial laboratories.

Do not think that there is any marked difference in talent or knowledge between applied physicists and those engaged in natural science.

If one likes, one can divide applied physicists into theoreticians, experimenters and hardware men. It is true that the measural experts play first fiddle in the orchestra of applied physicists as a very substantial body of applied research is aimed at creating new apparatus, improving existing tools and devising new methods of measuring the most diverse physical quantities.

Many applied physicists never engage in research. They work with complex instruments which they use for daily analytical or control work.

The swift growth of applied physics has led to the branching off of certain disciplines from it. Electronics, automation, power industry are all essentially branches of applied physics. They have diverged so widely that the specialists in them are now prepared in different colleges. Yet it is hard to draw a clear-cut line between them. As often as not, graduates of a university physical department and an electronics department in a technological college work at the same problem. Therefore I hope that the reader glancing through these pages will not enter into an argument with the author as to whether this is applied physics or some other science.

AND SO, CONCERNING PHYSICISTS EMPLOYED BY...

...**industry**. Imagine a modern factory that manufactures aircraft engines. It is a giant enterprise employing some 20,000 workers. It has shops that manufacture crankcases, crankshafts, bushings, connecting rods. Streams of parts

converge in the assembly shop, where they are joined together into engines. A finished engine is tested on a special stand and a paper is duly signed testifying to its excellent quality, after which the engine can be mounted in a plane.

Successful stand tests are a good guarantee of quality, but not a 100-per cent guarantee. And this is an aircraft engine a breakdown in which may cost many human lives. To make this as unlikely as an earthquake in Moscow, all the other shops in addition to the assembly shop must guarantee the quality of their output. The people making connecting rods must guarantee every single one of them; a flaw in a bushing is unthinkable; the slightest scratch on a crankshaft or hidden cavities in the body of a crankcase are impermissible.

A rigid system of control is essential to fully guarantee the quality of every single part. Various physical methods are used for this. X-raying reveals nonhomogeneities in metal. The keen eye of a physicist inspecting an X-ray picture or scanning the glowing screen showing all the parts can discover the smallest cracks or inclusions.

Next to the machine on which crankshafts are made stands an instrument for detecting the smallest surface fissures by the method of magnetic defectoscopy. The crankshaft is magnetized and oil containing magnetic particles is poured over it. The oil flows off and the particles adhere to the places on the metal surface where invisible cracks are located.

Other shops also have magnetic instruments. Incorrect heat treatment or deviations in the

thickness of a soldered layer are revealed by a property called magnetic susceptibility. The physicists' task is to develop various swift, convenient, precise methods of measuring this quantity with due account of the shape of the part and the materials from which it is made. And each new part presents new problems.

Responsibility for the functioning of the X-ray apparatus and developing new X-ray methods rests with the factory X-ray laboratory. The magnetic laboratory is responsible for all magnetic measurements in the shops.

Every metallurgical works must have a spectroscopic laboratory. Spectral analysis is most effective in continuous control of the composition of alloys manufactured in the smelting shop. Every alloy must meet rigid demands. The technological specifications list all the required additives, whose content must lie within certain limits, for example, no more than 3 and no less than 2.5 per cent. The list also states the permissible limits of undesirable additions, say, no more than 0.01 per cent.

Perhaps some readers may draw for themselves the picture of a shop foreman sitting before a scales with the technological specifications in his hand.

"Weigh 25.17 kilograms of copper," the foreman orders. "Now 3.25 kilograms of silicon."

This picture has not the slightest resemblance to reality. The charge of a smelting furnace includes a variety of raw materials and scrap, so that the composition is never known exactly in advance. As soon as the charge melts information about the composition is required. A test

sample is dispatched by pneumatic mail to the laboratory. Within seconds it is clamped in the spectrograph. A turn of a knob switches on the voltage and one tip of the sample is heated to a temperature higher than that of the sun. The metal vaporizes and its atoms glow in the flame of the arc: each variety of atom in its own inimitable way. The light radiated by the atoms falls on a glass prism, which spreads out all the colours to form a spectrum. Various lines appear on the spectroscope screen, on photographic plate or a TV screen.

Each sort of atoms has its characteristic representatives in the grille of spectral lines.

"Line of iron too strong! Reduce iron content," the spectral analysis laboratory reports, and the results of the analysis are given over the telephone.

There are many other laboratories in which physicists work. At many factories the physical department may account for from one-third to one-half of the central laboratory facilities.

Work in the physical department of a large factory with a diversified and changeable range of output requires inventiveness, acuity and comprehensive knowledge. In addition, as was mentioned in another place, physicists must know not only physics but also the domain it serves.

...**medicine.** I don't know whether official documents list such a term as medical physics. If they don't, I am sure they soon will. With each passing year physical methods of measure-

ment are finding greater and greater application in diagnosis. Intricate physical instruments are appearing in operation rooms. Only in medical practice is there still no appreciable penetration of physics. The quartz lamp and d'Arsonval apparatus still remain the main tools of the physical therapeutists. It may be my view as a non-specialist, but I must say that the only innovation I have noticed is the electrical sleep apparatus. Electrodes are attached to a patient's head and a simple installation sends rhythmic pulses that lull to sleep a person suffering from evil insomnia.

It is not hard to understand why physical methods very quickly occupied a leading position in diagnostics. A great number of different physical processes take place in the body, and all of them can be investigated by objective physical means. For example, microphones connected to amplifiers can measure all the beats and murmurs of the heart, while other instruments break the sounds down into an oscillation spectrum. The spectral curves of an ailing and healthy organism, as is known, are quite different. I recall that in Asia physicians distinguish up to one hundred different types of heartbeat. Prolonged, thorough feeling of the pulse is a basic diagnostic method. Since it is, evidently, not easy to learn this type of diagnosis it would seem logical to entrust the task to an acoustic analyzer.

Nowadays measuring blood pressure is no more difficult than measuring temperature with a thermometer. The results of such measurements, though, are very approximate. Why not invent

some method of measuring the blood flow through different arteries and veins which would register the slightest deviations from the normal?

The method by which gastric juice tests are made is indeed barbarous. I had one such test made once in my life — and won't any more. The procedure of swallowing a long rubber hose has impressed itself too vividly on the memory.

Couldn't we have, instead, a small device which the patient would swallow and which would make tests on the way down the gullet, relaying the results to the physician by radio? It may sound fantastic, but I have heard that such suggestions have been made in full seriousness, and in fact they have already passed the drawing-board stage.

The application of tracer atoms for diagnosis holds great promise of success. Counters are capable of detecting infinitesimal quantities of radioactive substances at a distance, so infinitesimal that they are of not the slightest harm to the organism. The rate at which the body metabolizes this or that element and its movement through the body can be easily traced by this method.

The contractions of the heart are associated with potential changes measurable by an electrocardiograph. Many years of observation have enabled doctors to correlate the curves of an electrocardiogram with pathological states of the organism. For the method to yield all its potentialities it must be placed on a theoretical foundation, the picture of jagged peaks and troughs must be explained.

The curves of the electrical activity of the brain obtained by applying electrodes to the skull could tell us much about the state of the nervous system. Could, if we learned to decipher their mysterious patterns. We are still a far way off from this goal, though experimental material is being accumulated at a high rate. Brain potentials immediately react to every sensation: the taste of salt or sweet on the tongue, music or noise, light signals of various brightness and colour. Every sensation affects this as yet mysterious carrier of information. Different parts of the head react differently to external stimuli. Healthy and sick people produce different wave patterns.

At first the sight of infinitely different brain waves may discourage the observer. An erratic curve beats and pulsates on the oscilloscope screen. One can see how it changes its shape as the brain responds to different events. How can one hope to unravel the message contained in the jagged succession of ups and downs?

Recently some substantial progress was registered in analyzing the mysterious waves. The electric potentials were fed into a computer ingeniously programmed to sift off all random, accidental peaks. The purpose was to extract from the waves only the regularly repeating patterns. As a result it proved possible to correlate characteristic brain wave fluctuations with specific external stimuli. These studies, which have only just begun, hold promise of major breakthroughs. Perhaps within several decades we shall be able to directly interrogate the nervous system about its health.

Quite a few fine achievements of surgery would never have been attained without the cooperation of physicists. You have surely heard of the remarkable operations performed on the heart. In some cases they are possible only when the temperature of the body is lowered substantially. This is achieved by different methods. For instance, the blood stream may be diverted, passed through a cooler and returned to the body so many degrees colder. Other methods of temperature control employed by physicists have been placed at the service of medicine.

Nowadays physicists have plenty of scope for work in medicine. The instruments and devices they build must be simple to operate and work well in the hands of doctors who need not know the whys and wherefores of their functioning.

Usually it is the hardware men in physics who find work in medicine. Extensive knowledge of experimental physics, knowledge of the techniques of all physical methods, engineering ingenuity, clever hands and special intuition enabling one to choose the most efficient and simple out of a number of possible solutions — these are the requirements such a researcher must satisfy.

...**linguistics.** Does the author mean to say colleges where people study grammar and syntax, analyze sentence structure and compare the roots of words in different languages describing the same concept will ever have any use for a physical laboratory? Will they? They already have! An example is the Moscow Institute of Foreign

Languages, which has been employing physicists for quite some time.

We could, to begin with, recall that language disciplines include phonetics, the study of speech sounds. Every student of foreign languages knows what a major stumbling block is pronunciation of a strange language. If you don't learn to speak the words correctly no one will understand you. Equally important is training the ear to understand strange accents. Perfect knowledge of a language consists, first of all, in understanding the speech of foreigners.

To learn to understand a foreign language by ear and discover the secrets of correct pronunciation the foreign languages scholar must know phonetics. It isn't hard to see that the study of phonetics can be elevated to an entirely new level with the help of acoustic spectral analysis.

The oscillation frequency of sound waves produced in the air by talking people of every race and nation lies approximately within the range of 300 to 5,000 hertz. It is known that any sound is objectively defined by its spectral composition. In the cold language of physics the difference between a shriek of panic and a melodiously sung note or exclamation of delight lies only in the different intensities of the oscillation frequencies represented in the given sound wave.

There exist remarkable complex physical instruments — sound analyzers — which are with equal ease capable of translating into the language of numbers the English dental fricative "th" and the French uvular nasal "in".

Different people, of course, possess voices of different pitch, hence an engrossing task is the quest for the common features that characterize the "average" pronunciation in a foreign language.

The tone of voice also affects the acoustic spectral curve, and it may vary substantially for one and the same person, depending on his mood or meaning. Thus the same word may sound differently. A change in inflection may completely alter the meaning of a sentence. How is emotional colouring reflected in the acoustic spectrum? This interesting linguistic problem has only recently become the subject of investigation.

Many new problems emerged in linguistics when systematic work on machine translation from one language to another began. Before a computer could be taught the various operations it had to carry out it was essential for the programmers to get down to the very bottom of their task. Incidentally, in other fields as well as in linguistics the need to establish the strictest order in one's thoughts has always been of the greatest use to researchers.

When the mathematical linguists undertook to teach machines to translate they discovered that they first had to understand for themselves the ways and means in which an idea finds expression in words. Why we say something like this and not in some other way, why word order is in some cases fixed and in others arbitrary. For instance, "He took a cup from the table" and "A cup he took from the table". The second sentence means the same as the first, but it's

worse. The machine must know why. And in telling the machine why we must naturally ask ourselves what makes the second variant worse.

In their quest for a language suitable for conveying the most complex ideas the linguists established that the rules of such a language can be many times simpler than the rules of living speech. The question at once arose whether there is any basis for the endless number of inflections the same idea may be given by playing with words.

In studying the laws of sentence construction linguists come to the conclusion that nature cannot be accused of being too extravagant. It has been found that the richness of the language, the flexibility of speech and the diversity with which the same thought can be presented is a method of unloading the memory. It has been shown in recent works that a simplification of the language would require an enlargement of memory "depth".

Thus, in a language with simple, rigid rules of speech a necessary word is pigeon-holed in the brain in such a way that access to it can be gained by one route only. It lies tucked away at the end of a cul-de-sac in a maze of streets, and there is only one entrance to it: second turn to the left, then third to the right — and no other way.

In real languages the requisite words are close to the surface and many roads lead to the pigeonholes in which they are stored. One can imagine how much the search for them is simplified when one can choose any of several roads. Thus an overly deep memory may be a drawback, and

the theory I have mentioned declares that there is no need for it.

"These may all be very stimulating fields of research," the reader may say, "but where do physicists come in?"

But surely, to investigate the question thoroughly one must possess a physicist's mentality. Mathematical linguistics can be classified among the natural sciences for the simple reason that its investigations are inseparably linked with questions of analogues in the functioning of the brain and electronic computers.

...**sciences of the earth**. He tells fascinating stories of tropical sunsets and Polar lights, of uninhabited atolls and picturesque desert oases. People listen raptly and envy him. Just imagine all the places this traveller and geographer has seen, all he's experienced. What interesting things can a physicist, hemmed in by the walls of his laboratory, have to tell?

This view seemed only natural. Undoubtedly, not so long ago travellers had a monopoly of describing beautiful distant lands and remarkable natural phenomena. But times have changed. Today physicists rise aloft on balloons, sail in submarines, descend into volcano craters. They winter on the North Pole, penetrate into the interior of Antarctica and circumnavigate the globe. And the geographers? They are compelled to spend their time in the quiet of laboratories to brush up their physics. Otherwise they risk being left behind by the physicists embarking on expeditions.

Nowadays the globe is one vast physical laboratory and experiments in it sometimes involve such exotic trips that could well be the envy of famous explorers of the past. Marine geophysics probably holds pride of first place in this respect.

A small wooden sailship rides the crests of ocean waves. No, this is not a relic of a 19th-century pirate fleet. It is the nonmagnetic schooner *Zarya* in which Soviet physicists cruise the high seas to study the earth's magnetic field. One cannot surprise the physicists on board the *Zarya* with tales of strange lands, tropical cloudbursts and tropical heat or the customs of the natives of Polynesia.

Another floating physical laboratory is the ***Vityaz.*** The physicists on board are not just members of the expedition: they tell the skipper whither to steer the ship in accordance with their plans of research. And these plans are as interesting as they are varied.

Here is but one example.

The ocean deeps surpass the height of Mt. Everest. Is the water at the ocean floor, many kilometres from the surface, at rest or in motion? Can a particle of water ascend from these depths to the surface or not? If it can, how long does it take?

"What strange questions," the reader may say. "Who needs to know the answers?"

Back in the forties it was suggested that the waste products of the atomic industry could be dumped on the ocean floor. But what if the waters carry the radioactive poison from the deeps to the surface? What will happen to fish? And to people eating the fish?

Evidently, the time it takes this water to rise to the surface is of crucial importance. If it is substantially greater than the decay of radioactive substances, then by the time they reach the surface they will be harmless. But if not? Who then will venture to dump radioactive waste into the ocean?

This, naturally, bred the scientists' interest in questions of the deep circulation of ocean waters. Both experimenters and theoretical physicists had to work hard on them.

The experimenters had to learn to measure the velocity of currents several thousand metres below the surface, taking into account that they may be no more than a few centimetres per second. As exact measurements are difficult, if not impossible, theoreticians were called in to cooperate in calculating these deep currents. The physicists' answer to the question was of vital importance for the destinies of the ocean.

The experimenters invented neutral buoyancy floats that could be lowered to great depths. Their displacements over long periods of time revealed that considerable mixing of water can take place at the bottom of the ocean. This lit the first red light against the dumping of radioactive waste in the ocean. Although many theoreticians still disagree on the final results, they have nevertheless arrived at the important conclusion that particles of water can rise from the bottom to the surface of the ocean in a time comparable with the decay time of the most radioactive elements in waste products.

Thus, the theoreticians and experimenters established beyond all doubt that there does

exist the danger of contaminating the ocean if it is used as a dump for radioactive waste.

Another wandering profession for physicists is geological prospecting.

There is a branch of geophysics called gravimetry, which treats, as its name implies, of the measurement of gravity. In different parts of the globe the same weight weighs (that is, is attracted by the earth) differently. This difference is detected with the help of remarkable precision instruments as, for example, a quartz torsion balance. It consists of a horizontal quartz thread to which a delicate beam is sealed. The measured weight causes the beam to wind the thread slightly. The balance can measure forces of millionths of a gramme.

Physicists take their instruments on distant travels to places where they observe the behaviour of gravity. A change in the force of gravity against the norm for a locality indicates the presence of ore beneath the surface. Local anomalies in the force of gravity are, to the physicist, like a magic wand that points to hidden treasures.

Such methods of prospecting are of value in the search for oil. Gravitational methods readily detect underground salt domes (the force of gravity decreases over them), and salt is frequently associated with petroleum. Many oil fields of Kazakhstan were discovered in this way.

It is, of course, interesting to be a geophysicist and to be able to travel a lot. But one day one may get tired of roaming about. What must one do then? Change one's profession? Not at all: geophysicists can work successfully in laboratories.

Perhaps the juxtaposition of the words geophysicist and laboratory may appear strange. Geophysicists study nature, their instruments must be installed in the field to detect various features in the flow of rivers, the blowing of winds, the flashing of lightnings, things which cannot be determined within the four walls of the laboratory. And yet, even leaving aside the work required to build and study complex apparatus, a geophysicist can devote his life to the study of the universe by modelling natural processes. More, in some cases this is the main method. It is, of course, exciting to catch real lightnings with instruments in a storm. But storms are not all that frequent and, besides, nature never presents us with the "pure" phenomenon but rather with an agglomerate of incidental and fortuitous factors which obscure the main thing. Therefore it is impossible to gain a clear understanding of the laws of lightnings without studying artificial lightnings in laboratory conditions. A model installation can be used to test the role of various factors before verifying the rules derived in natural conditions, where many factors act simultaneously.

The superhigh temperatures reigning on the sun, the superhigh pressures of the interior of the earth, high vacuum or greatly ionized air can all be reproduced in laboratory conditions where, by studying the features of these unusual conditions, one can approach important conclusions useful to the sciences of the earth.

Chapter 15

PHYSICISTS AND LYRICISTS

...in which the author finds it impossible to remain aloof in the debate concerning the attitude of physicists towards art and of poets and artists towards physics. In our press the problem was discussed under the heading "Physicists and Lyricists". Abroad it is known as "the problem of two cultures".

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A man who daily listens to the radio and watches television, travels in jet planes, sees photographs of the reverse side of the moon and wears nylon clothes doesn't have to be told that the achievements of physics have become part and parcel of his life. Nor shall I do this. But the invasion of physics into everyday life is not restricted to new things; it also involves new ideas. Little by little the physical mentality is making headway in the sphere of our spiritual life. As I see it, this "invasion of ideas" is of no less, if not greater, interest than the "invasion of things".

To approach events in the world of men from the same standpoint as phenomena in the world of atoms, molecules or cells means, as I tried to make it clear in the foregoing discourse, to seek the objective laws of repetitive phenomena.

To explain a phenomenon means to show that it derives from a general law of nature. The physical method of examining events dis-

regards all that cannot be measured or computed. For the physicist there is no God insofar as there is no way of proving it by measurements or computations. In this sense the notions of an electron's path, simultaneity of events, and God are related; i. e., they are meaningless.

Scientific thinking rejects unprovable statements. Every declaration must either be proved by experiment or logically deduced from indubitable premises.

The natural scientist describes the world as it is. It has no use for words like "necessary", or "good". Insofar as these words exist in the world of human relations, the researcher attempts to explain them, that is, to prove that necessary or good acts are a logical consequence of certain relations between men or between men and their environment, just as a satellite's motion on a specific orbit is a consequence of universal gravitation.

Obviously, such an approach to events in the life of man and society will find many opponents. The scientific approach used with such brilliance by Marx to explain historical events was opposed by bourgeois philosophers and churchmen for the simple reason that, if events in the world of men are regarded as a process obeying certain laws, then this leaves no place, not only for God, but for "enlightened" rulers as the creators of history, as well.

Modern natural science goes farther along this road and seeks the ways of objectively describing and explaining the behaviour, not only of society as a whole, but of its individual representatives.

Since the road of science passes through the search for general laws, then, as applied to man, this at first appears to mean the absence of any interest in the thoughts, acts and emotions of the individual, that is, in the principal themes of art. This was utilized by many thinkers to speak of the abyss that lies between the worlds of science and art.

This is not a new argument. In our time the "problem" has been posed by the English author Sir Charles Snow, it has lately been discussed by Robert Oppenheimer, Aldous Huxley, Lionel Trilling and many others. Independently of arguments that went on abroad, a heated discussion filled the pages of our press for some time. It was sparked by workers of art worried that the role of "engineers of human souls" was slipping from their hands. Their fundamental thesis was the alleged spiritual poverty of people ignorant of art. Representatives of science sharply objected to accusations of the spiritual poverty allegedly due to their preoccupation with natural science. Ultimately both parties came to the conclusion that the argument was based on a misunderstanding and that, in fact, there were no grounds for it. And yet I think it useful to devote a few pages to the "invasion" of physics into art.

Whatever one's views, the very fact that people are discussing the subject of "Art and Science" is indicative of the ever-growing impact of physics and physical thinking on the life of intellectually minded people, hence it concerns the subject of this book. The problem is an extensive one, but I should like to dwell on two questi-

ons: Do people of a strictly scientific bent have any special attitude towards art? To what extent does the development of science affect contemporary art?

It is with some qualms that I proceed to answer the first question, of the attitude of physicists towards art. That tastes differ widely is known only too well. Not so long ago our cinema journal *Ekran* floated a questionnaire amongst its readers. The number of diametrically opposite views was so great that the magazine rightly concluded that a much more comprehensive poll was required to determine the statistics of opinions. This is only natural, since national differences, social conditions, age and family upbringing, superimposed on natural inclinations, create people so unalike that it would be, to say the least, strange if their views on art coincided.

Speaking for myself, I must say that I have not conducted any polls among my colleagues and therefore, in venturing on certain generalizations, I warn that if anyone disagrees and challenges me I shan't pick up the gauntlet.

To begin with, the physical mentality develops a habit of trusting one's own view more than those of others. Therefore one should treat men of science with understanding when they lay claim to independence in evaluating works of art and refuse to submit to the hypnosis of other people's judgements.

Another trait in the tastes of physicists which I consider more or less characteristic is their insistent requirement that a film, play or novel possess an intriguing plot. As a rule physicists are not impressed by references to the author's

subtle psychology, the depth of his philosophical generalizations, or his remarkable form. They like adventure, fantasy, crime novels.

This predilection may be characteristic not only of physicists but of all people devoted to their work. If a book fails to move you, your thoughts are sure to wander to your beloved work. There may be another explanation of this preference for novels with tightly wound plots: the natural thirst for activity which finds no outlet in your profession.

From this, probably, springs the view that scholars are left unmoved by works of art distinguished not so much by the plot as by their emotional intensity. Though if such a work is true to life and is devoid of affected pathos or sentimentality it will be sure to find the normal number of admirers in the academic community.

Now about music, poetry, painting, about works of art through which the artist addresses the human heart, "bypassing the mind".

In most cases people who have devoted themselves to science derive great satisfaction from learning to listen to poetry or music and view paintings. My impression is that physicists are no exception in this respect. And their inherent inquisitiveness helps them penetrate deeper into the domain of art.

To begin with, they wish to understand what makes so many people stand for hours before a Roerich or a Gauguin, listen devoutly to the verses of Blok or Akhmatova. When this initial curiosity passes the harmony of words, colours and sounds penetrates straight to the heart.

He who likes to reason logically and knows how perhaps loses somewhat in spontaneity of perception. As far as I am concerned, an artist only too often fails to speak straight to my soul.

But it would be wrong to think that this admixture of rationalism to the emotional juices that digest the gifts of art impoverish man's spiritual life. The aesthetic delight derived from a work of art, far from being impaired by the presence of reason, is catalyzed by it. I value works of art not only for the immediate emotions they arouse but also for the thoughts they have kindled (whether the artist wanted it or not), for these thoughts are, in turn, a source of excitement.

I think that such perception of art is characteristic of people of my profession.

All this is directly linked with the aesthetic perception of scientific facts and laws mentioned in the discussion of the beauty of laws and equations. An ability to be moved by a beautiful idea and its mode of expression naturally complements the immediate perception of a work of art. Therefore I find it impossible to accept the view that rationalism impoverishes and dessicates man.

Besides its function of entertaining, moving, affecting the emotions, art also has the task of fostering ideas and rules of behaviour. Some people regard this as its main task. Mature people consider themselves educated and rarely consciously turn to art in search of living truths. Art, like nature, educates imperceptibly, bit by bit.

Physicists, for whom ratiocination is a profession, are possibly less prone to seek pure ideas

in art than representatives of other professions, For in truth, man is a piano with many strings. but life plays its pieces within one octave. The untouched strings also wish to sound, this is an instinct of life.

Music, poetry, painting pluck the strings neglected by everyday work, the routine of life, the daily grind.

This, it seems to me, is the main thing.

The second question I wished to discuss is the influence of science on art.

That this influence exists can be seen in the growing interest of artists in science. What are the causes that compel poets to attend scientific seminars, read the magazine *Science and Life* and heed the voice of physicists with due attention and appreciation?

The attitude towards science changed before the eyes of people of my generation. When I was a boy an educated man was expected to have more than a smattering knowledge of painting, the theatre and literature; he need not know a thing about the laws and workings of nature. It seemed obvious that art was quite capable of providing an understanding of the meaning of life, the rules of human behaviour, human relations, the relationships of man and society — in short of all that constitutes the substance of life.

I remember only too well the almost contemptuous lack of interest in natural science displayed by the humanities students in my student days. And their utter confidence that knowledge of general human truths had nothing in common with problems of natural science. This picture

has changed since then, and today young physicists confidently harangue the crowd and their contemporaries in literature and history listen to them respectfully.

Nowadays poets and artists find it essential to gain some understanding of physics and biology. Workers of art meekly accept accusations of ignorance of the fundaments of relativity theory or quantum mechanics. Today it is just as impermissible to call Einstein a mathematician as it is to call Picasso a musician (an example of such ignorance was cited by the Rector of Moscow University).

In short, poets study natural science. What for?

If it would have been possible to use H. G. Wells's time machine to transfer several thousand babies from ancient Greece or Rome to our time, their progress in education and upbringing would hardly differ from the achievements or failures of contemporary children. Genetic changes is a slow process and several millennia is no more than a second for evolution. Aristotle and Democritus would probably wind up in a school for especially talented young people and study quantum mechanics successfully. Sophocles and Aristophanes would write two-act psychological plays, while young people without any talent for writing or aptitude for drawing would successfully master the trades of radio technician or pattern-maker and on their days off would attend football matches with the same avid interest as they would have gladiator fights in their former lives.

In the time we are capable of probing with the help of historical documents, the nature of

man and his passions have undergone no change. Yet representatives of different ages differ greatly. These differences are rooted in social environment, social relations — that is, in education and upbringing. But in all epochs man has always remained man, and it is confirmed beyond all doubt by art.

Eternal themes constitute the core of art: love and jealousy, friendship and war, the contradictoriness of human nature... That is why the tragedies of Shakespeare, nay, of ancient Greece, are staged with unfailing success. The verse of Ronsard moves us just as it did the great French poet's contemporaries. The statues of Praxiteles and the sculptural portrait of Queen Nefertiti continue to please the eye and serve as standards for judging the talent of modern sculptors.

But every new generation of artists strives to express the world in new ways. While remaining within the circle of eternal themes, they seek to achieve this through new forms and the intertwining of undying topics with new social relationships.

A genuine artist desiring to contribute something of his own to the eternal themes of art avidly investigates contemporaneity, seeks the colours of the epoch that could give his work inimitable hues.

But the features of the epoch are not restricted to social conditions. The advances of science have led to technological discoveries that affect the destinies of the world. The gains of natural science have compelled people to reappraize many moral values. One need but recall the proofs of the material nature of spiritual life.

Obviously, art cannot pass by these changes. The idea (perhaps subconscious) that one must seek in this influence of science on life new, untested colours which would contribute to the inimitability of the eternal themes of art, seems only natural.

The results of this attention of workers of art to science are already apparent. Positrons and neutrons have appeared inverse. Abstract canvases are given names like *The Lorentz Transformations*. Theatres stage a play called *Physicists*. In many novels the protagonists are workers of research institutes. More and more science-fiction novels are appearing. But these are merely the superficial signs, and the fundamental revolution in art is evidently still to come.

It would be wrong to think that science can gain a foothold in art only in the field of subject-matter. After all, the artist's purpose is to convey his specific sensations and perception of reality to his readers or viewers.

The invasion of physics into art should lead to the development of a new poetic view of things and people. Sooner or later artistic perception of the world will take place through the prism of scientific thinking. Already now there are indications that one day scientific thinking will affect poetry.

When you know the nature of things it is impossible to write of them as you did when you were ignorant. The English poet John Keats is said to have hated the great Newton for explaining the rainbow and thereby severing the poetic links between it and heavenly forces. The scientific explanation, he thought, had impoverished poetry. Today we know that this is

not so. But what is important is the confession that, once you have the knowledge, it is impossible to ignore it either in perception or in expression by artistic means.

Is Keats (as well as his numerous contemporary and present-day supporters) right that science takes the poetry out of life? I think not.

He who assumes that science hinders poetry would have it that poetry ends where the connections between phenomena begin to be felt or where the underlying causes of human passions and emotions become apparent.

Would it not be more correct to claim that knowledge of the nature of things should give rise to richer poetic images? Knowledge which his forerunners lacked can alone help the artist discover new words and colours to produce genuinely contemporary works of high perfection.

Soviet writers, poets and artists display a much greater interest in science than their foreign counterparts. This is only natural, for they are brought up in the realistic tradition. The indifference to scientific knowledge characteristic of Western servants of the Muses results in quests for innovation restricted to form alone. The absence of a plot, predilection for form, the "new novel," in literature — all these are upshots of the failure of the search for innovation along the old roads of artistic development. The victims evidently didn't stop to consider the boundless vistas opened up to creative inspiration by the phenomenal advances of natural science. They do not understand that as long as our views on nature and man continue to evolve realism will never exhaust itself.

* * *

Many pages and chapters in this book were specifically written for the benefit of young readers pondering their future profession. I set myself the goal of telling what the profession of a physicist comprises, to show the truly boundless opportunities the science and its applications provide in practically every walk of life. To be sure, nowadays physics as a profession does not need publicity. On the other hand, the knowledge of physics pupils obtain at school usually provides an erroneous idea of the profession.

The universality of physical methods of research and of theories usually remains outside the framework of the school course. The youth or girl choosing a profession is often torn by apparently conflicting desires. It is only natural for a socially conscious citizen to wish to work in the field where his country needs him most. That is why so many people are attracted by chemistry. Or perhaps physical instruments possess more attraction than chemical test-tubes. Or one wishes to travel, to see the country, and at the same time has a predilection for exact mathematical calculations.

That is why I felt it necessary to show that these and many other desires, far from being contradictory, can naturally complement each other.

If I should try to put the purpose of this book in a nutshell I should say that it was written to show the role and place of physics in modern culture.

In the last few decades the importance of science in public life has grown tremendously. It is hardly surprising that to this day a large proportion of Western intellectuals have but a vague idea of the essence and content of scientific work. One often faces judgements distinguished by truly remarkable one-sided approaches to things. The arguments against physics are numerous: radioactive fallout, the atomic bomb, missiles — without science man would have none of these "gifts". These arguments are not new. Something similar was said against automation. It should be apparent that the harm science sometimes yields is due to the nature not of science, but of capitalist society.

Coming very close to the denouncers of science are those who would divorce science from its practical applications, and those "pragmatists" who view science as nothing more than a means of multiplying material values.

With all these extreme points of view in mind, it seemed expedient to dwell in detail on the division into applied and natural sciences and show their interaction. This was my primary task.

We are witnessing the assertion of a unified view on the nature of living and inanimate matter. The boundaries between different chapters of natural science are vanishing. The notion of special nonmaterial carriers of chemical and biological processes is collapsing.

The achievements of science testifying to the subordination of all nature to common physical laws frequently encounter the resistance of idealistically-, or simply narrow-minded specialists.

School education erects high fences between scientific disciplines.

That is why I thought it important to dwell on the building of a common foundation — the laws of physics — for all natural science.

This was the second task I undertook to solve.

This book is written for a wide circle of readers. Was I justified in counting on the interest of so wide an audience?

Man is inquisitive, he has a keen interest in all aspects of social life, and because he has learned to link very different events together he has become the master of nature. These splendid qualities of man give me confidence that the problems treated in the book will be of general interest. This compliment addressed to my future readers is not designed to curry their favours. If the book is bad the loftiest of themes and the author's best intentions are incapable of saving it.

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